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AGRICULTURAL RESEARCH INSTITUTE  
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# PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

# A C C O U N T

O F T H E

Present Undertakings, Studies, *and* Labours,

O F T H E

# I N G E N I O U S,

I N M A N Y

Confiderable Parts of the WORLD.

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VOL. XLVIII. PART I. For the Year 1753.

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**PHILOSOPHICAL  
TRANSACTIONS.**

**VOL. XLVIII. PART I.**

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# PHILOSOPHICAL TRANSACTIONS.

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Page 201, for *Art. XXXII.* read XXXI. P. 34, Note (1) for TAB. I. read TAB. II. In Sheet (Y) for 173, read 169. For 176, read 172. For 177, read 173. And for 180, read 176.  
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- I. *An Account of an extraordinary Stream of Wind, which shot thro' Part of the Parishes of Termonomungan and Urney, in the County of Tyrone, on Wednesday October 11, 1752. by Wm. Henry, D. D. Rector of the Parish of Urney: Communicated by the right honourable the Lord Cadogan, F. R. S.*

Read Jan. 11. 1753. **T**HE air for the whole day was serene and calm; sometimes a gentle breeze from the south-east. About four of the clock in the afternoon, the sky seemed to open; and there was a flash of lightning from the south-east. In the space of half an hour after, there was heard thunder, as at a great distance, from the same point. About five the sky was a little overcast with clouds; but the air continued in a dead calm. On a sudden there was heard a violent rushing noise; the sky seemed to open, and emitted a flash of lightning, but no noise of thunder; and a stream of wind instantly ensued, the violence of which nothing could resist.

This stream of wind, so far as can be traced by the effects, arose from a glin. called Allgolan, and continued its course for three miles from south-east to north-west. The violent current of it seemed to be confined to a space about 16 feet in breadth, and the whole body of the air in motion did not exceed sixty feet, as may be computed from some of the following particulars, which happened in the little  
A
village

village of Lisnacloon in the parish of Termonomungan, and the edge of the parish of Urney.

At the distance of a mile to the south-east of this village, it cut a line thro' several clamps of turf, which were standing in a bog, and tumbled down all the clamps in this line. Thence it cross'd the river Derge, in the same line, and dash'd up the water with great noise and violence, as was observed by John Kyle, who has mills on the river, and several others. Thence, in the same line, and at the space of half a mile, it took the village of Lisnacloon, where there are 13 dwelling-houses, beside office-houses, belonging to farmers and cottagers, scattered irregularly.

1st, It dashed down an hay-stack belonging to Wm. Montieth, which was the first object in its way; and stripped intirely twelve feet off the roof of his dwelling-house.

2dly, It knocked down Henry Carólan's turf-stack, and carried some of the turf above 300 yards over the cabbins into the fields.

3dly, At the distance of 69 paces, it took Henry Crawford's house. Full in the broadside of which, it stripped 59 feet, leaving each of the ends, above and below the stream of air, quite unmoved. This particular points out its utmost breadth.

At the back of this house it overset an hay-rick, which stood in its line; but did not ruffle any of the corn-stacks, which stood within a few yards to the north side.

4. It knocked off eight feet of the roof of Solomon Folliot's kiln, which stood in its line.

5. It

5. It levelled 55 feet of David Montieth's garden-ditch.

6. It levelled, in the same direct line, Wm. Folliot the younger's hay-stack, which stood south-east from his house.

7. It burst with incredible violence thro' his cow-house, and cut a passage of 16 feet quite thro' it, and carried some of the ribs of the house before it 400 yards into the field: The rest of the house was a little ruffled. His wife, who was gone into the cow-house a minute before, was knocked down by one of the ribs falling. She declared, that it was a dead calm the minute before; when, on a sudden, she saw a flash of lightning, and heard and felt the violent storm; but heard no thunder.

Old William Folliot, aged 93, who was walking in the field, at the back of the house, was blown down, and grievously bruised. He saw the lightning, but heard no thunder.

Solomon Folliot, being in the same field (but out of the line, in which the stream of wind passed) felt no wind, but heard a mighty rushing noise, and saw the timber, thatch, turf, and dust of the houses, fly by him, at the distance of forty yards. He saw a flight of rooks dashed down in the same field.

In this village are several other inhabited houses, both on the north and south sides of the course of this stream, none of which were in the least ruffled. The air continued still, among these houses; and the inhabitants stood astonished, on seeing the sudden devastation so near them.

After passing this village, the stream was continued in the same line, but with less violence, to a

large hill in the parish of Urney, which is called Muckle; and, on the north side of the hill, at the distance of a mile from Lisnacloon, burst open the door of John Ranking, a weaver, and broke down a web in his loom. As at this last place it entered a large bog, which is extended for three miles, it could be traced no farther.

The time, in which this stream passed thro' the village of Lisnacloon, was about five minutes. It was succeeded immediately by a torrent of rain.

Having been informed of this extraordinary phenomenon, that I might have the more perfect knowledge of all particulars, I took with me two gentlemen, Dr. Michael Law, a physician of note, and the reverend Charles Rhea, on the 20th instant, from Strabane, and view'd and measur'd on the spot the course of this violent stream, as it appeared by the marks; and at the same time examined minutely the several inhabitants of the village of Lisnacloon, who were eye-witnesses of this fact; and from their united testimonies, and my own ocular observations, collected the above account.

Given under my hand this 21 day of October,  
1752.

William Henry, *D. D.*  
Rector of the Parish of Urney.

II. *An Account of a Book, intitl'd, P. D. Pauli Frisii Mediolanensis, &c. Disquisitio mathematica in causam physicam figuræ et magnitudinis Telluris nostræ ; printed at Milan in 1752. inscribed to the Count de Sylva, and consisting of Ten Sheets and a half in Quarto: By Mr. J. Short, F. R. S.*

Read Jan. 18, 1753. I. **I**T may be laid down as a rule in mix'd mathematics, " That the determination of no physical quantity be carried farther than the observations, or other mechanical measures, can bear ;" lest there follow this incongruity, of the conclusion being more extensive than the premises. It were absurd, for instance, in the resolution of a triangle, to compute an angle to the exactness of seconds, or a side to centesms of an inch, when, perhaps, the instruments used can measure no angle less than 10 minutes, or a side but to the exactness of a foot. The conclusions of arithmetic and geometry are indeed rigorously true, but they are only hypothetical ; and whenever the quantities, that enter any practical question, cannot be measured, but within certain limits, it were in vain to look for an answer perfectly accurate. The error of the instrument becomes itself one of the *data* ; and we must content ourselves to find the limits, which the quantity sought cannot well exceed, or fall short of, by such rules, as the great Mr. Cotes has left us in his excellent treatise on the subject.

2. In



2. In like manner, when any physical theory is deduced from observations, its accuracy will still be in proportion to that of the observations, on which it is founded. Sir Isaac Newton, we find, in computing the ratio of the earth's axis to its equatorial diameter, confines himself to a reasonable approximation, and to three places of figures (229 to 230); because, whether that ratio is deduced from the different lengths of isochronous pendulums in different latitudes, from the measurement of distant degrees of a meridian, or from both; the elements of the calculus can scarcely furnish a greater degree of exactness. And of the same judicious caution we have many other examples in the works of that incomparable author.

3. On the other hand, when observations and theories are brought together, and compared, nothing can be justly inferred against a theory from its disagreement with the observations, unless that disagreement is greater, than can be fairly imputed to the imperfection of instruments, and to the unavoidable mistakes of an observer; especially, if the difference should be sometimes in excess, and at other times in defect; or, according to some of the observations, should intirely vanish.

4. Altho' these rules, manifestly well-founded, have been followed by all the best writers, our author observes, that several ingenious men, both in France, and in Italy, have deviated from them; particularly in treating of the famous question concerning the figure of the earth. Some, with Messieurs Clairaut and Bouguer, attributing too much to the observations, that have been made, and taking them  
for

for absolutely exact, have concluded Sir Isaac Newton's reasonings on that subject to be faulty; while Father Boscowic, a Jesuit at Rome, making them quite loose and uncertain, thinks no argument at all can be drawn from them, concerning the earth's figure; far less in confirmation of the Newtonian theory.

5. In opposition to these two extremes, equally contrary to reason, as they are to each other, F. Frisi writes the treatise now before us; in the introduction to which he shews, 1. That, altho' the ratio of the axis of the earth to its equatorial diameter is, from M. de Maupertuis's operations in Lapland, and afterwards in France, that of 177 to 178; and by the theory only 229 to 230; yet the difference is no more, than what might arise from a mistake of about 60 toises in the measure of either of the two degrees, that are compared, or of 30 toises in each of them. Or, suppose the measure of the arcs to be exact, the same difference might be owing to an error of 4 or 5 seconds in the astronomical part. And such errors, or others equivalent to them, in a course of so many combined operations, our author looks upon as difficult to be avoided. But he adds, if the observations of M. de Maupertuis, and his fellow academicians, seem to differ from the theory, those of Messieurs Bouguer and de la Condamine exactly agree with it: According to whom, a degree at the equator, containing 56753 toises, and in latitude  $49^{\circ} 22'$  57183 toises, the difference of the axis and equatorial diameter comes out to be  $\frac{1}{219}$ .

6. In answer to F. Boscowic, and those who make no account of the observations, our author allows, that,

that, if they were such, as M. Caffini, and some other academicians, made in France, of the measure of a parallel of latitude, they could not be much depended on; that method being liable to several obvious inconveniencies. But he insists, that, with the excellent instruments, which were used, and, considering the distinguished skill of the observers, as well at the polar circle as in France, and at the equator, the error upon one degree of the meridian could not exceed 60 or 70 toises; which is a degree of exactness not only sufficient for the determination of the first question, *viz.* whether the spheroid of the earth is flat or long; but likewise to found an agreement between the observations and the theory, as near as can be expected or desired.

7. The work itself is divided into ten chapters :

- I. De observationibus circa telluris figuram hætenus institutis.
- II. De principiis et hypothesibus quibusdam.
- III. De rotatione corporum, et vi centrifuga.
- IV. De mutationibus ex motu circulari ortis.
- V. De attractione corporum rotundorum.
- VI. De comparatione gravitatis in variis homogeneæ sphæroidis locis.
- VII. De figura terræ.
- VIII. De gradibus meridiani et parallelorum.
- IX. De loxodromiis nautarum, de parallaxi lunæ, et aliis ex eadem theoria pendentibus.
- X. De theoriæ et observationum consensu.

8. In chap. I. we have a short history of the inquiries, that have been made into the magnitude and figure

figure of the earth down to the present times ; and the preference is justly given to the measurements of Mr. Norwood in England, A. D. 1635, and of the members of the French Academy of Sciences since that time. From these he gathers, that, within less than 60 or 70 toises, the lengths of a degree of the meridian are as follows :

	<sup>R</sup>	/	Toises.
Lat. 0	0	0	56753
45	0		57100
49	22		57183
53	0		57300
66	20		57400

9. Chap. II. contains an account of the principles, on which this theory is founded ; *viz.* the universal gravitation of matter, and the diurnal rotation of the earth. Our author mentions likewise the hypothesis of the earth's being originally in a fluid state ; but rejects it as precarious and improbable. He allows however, that, with regard to the present question, it is all one whether it was first a fluid or not, seeing the ocean is circumfused just in the same manner, and to the same altitude, as if the whole was still a fluid.

10. Chap. III. and IV. are employed in the doctrine of centrifugal forces, and their effect in changing a fluid sphere into the form of an oblate spheroid. In the former of these chapters, the author resolves, as usual, the centrifugal force of a particle into two others ; one, that acts directly contrary to the gravitation of the particle ; and the other a force in a direction perpendicular to it. And this last he considers  
B again

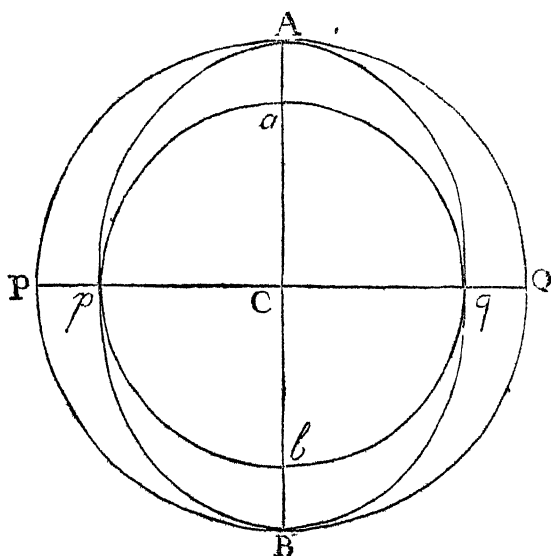
again as acting laterally upon the contiguous particles impelling them towards the equator. But the quantity of this force, when greatest at the octant, he computes to be only  $\frac{1}{668168}$  of the force of gravity; and therefore, says he, it may be safely neglected. The truth is, after the spheroid is come to be in a permanent state, and all its parts in *æquilibrio*, there is no longer any such lateral force at all; it being now intirely satisfied by the gradual contraction of the earth's axis.

11. The general contents of the following chapters are sufficiently expressed in their titles already given. Nor can we be more particular, without entering into a detail of algebraical operations, which would be improper for this place; and which is the less necessary, as the same things have been treated of by several other authors.

This does not, however, in the least detract from the merit of F. Frisi; who discovers throughout this work much acuteness and skill, joined with all the candor and ingenuity, that become a philosopher. And as he has not yet exceeded his 23d year, it may be expected, that the sciences will one day be greatly indebted to him; especially as we find him actually engaged in composing a complete body of physico-mathematical learning.

12. There is only, in his VI. Chapter, a criticism upon one of Sir Isaac Newton's demonstrations, in which we cannot agree with this ingenious author. And as this demonstration has proved a stumbling-block, not only to F. Frisi, but to many other learned men, we shall be obliged to consider that part of it, which has been mistaken, at some length, by the help of the following scheme:

In



In which let the ellipsis  $ApBqA$ , whose axes  $AB$ ,  $pq$ , are in any given ratio, as of  $m$  to  $n$ , have the circles  $apbq$ , and  $APBQ$ , inscribed and circumscribed to it: And if the figure revolves on the axis  $PQ$ , there will be generated an oblate spheroid  $ApBqA$ , with two spheres, the greater circumscribed to the spheroid, and touching it in its equator  $APA$ , and the lesser inscribed and touching it in the poles  $pq$ ; the solid content of the spheroid being the first of the two mean proportionals between the solidity of the exterior sphere, and that of the interior.

But if the figure revolves on the axis  $AB$ , there will be generated a prolate spheroid  $ApBqA$ , inscribed in the exterior sphere at the poles  $AB$ ; and circumscribing the interior sphere at the equator  $pqp$ , its solidity being the second of the above mean proportion-

als. So that if  $O$  and  $P$  stand for the solidities of the oblate and prolate spheroids, and  $S, s$ , for the two spheres;  $S : O : P : s \div$  are in the continued proportion of  $m : n$ . And  $S : P$ , or  $O : s :: m^2 : n^2$ . As  $S : s :: m^3 : n^3$ .

Or we may with Sir Isaac Newton conceive of the genesis of these solids as follows. 1. Let the sphere  $APBQ$  be uniformly compressed in the direction of its axis  $PQ$ , till that axis is diminished to  $pq$ , and the sphere changed into the oblate spheroid. 2. Let this spheroid be equally compressed in the direction of that diameter of its equator, which is perpendicular to  $pq$  and  $AB$ , or to the plane of the figure; and it will degenerate into the prolate spheroid, whose poles are  $A$  and  $B$ . 3. Let this last be compressed in the direction of its axis  $AB$ , till it is changed into the sphere  $apbq$ ; and, in each of these compressions, the solid space, which the body contains will be diminished in the ratio of  $m$  to  $n$ .

Now, as the determination of the earth's figure depends not only upon that of the ratio of the centrifugal force, by which a body tends to recede from the axis of rotation to the power of gravity; but likewise, upon the decrement of gravitation arising from the body's being in that rotation actually removed to a greater distance from the centre: it is not enough, that we know, from the experiments with pendulums, the centrifugal force at the equator to be about  $\frac{1}{289}$  of the force of gravity. We need, farther, two distinct propositions; one to determine the attractive force of a spheroid at its pole; and the other to determine its attraction at the equator. The first of these we have in *Princip. lib. 1. prop.*

91. and the second has been supplied by several authors. But Sir Isaac, who seldom does any thing in vain, found, that he could, by one of his artifices, make that 91<sup>st</sup> proposition serve likewise to determine the attraction at the equator, by the following argument:

Let  $G$  be the attraction of the exterior sphere at  $A$ ; and let the decrement of that attraction, when the sphere is diminished into the oblate spheroid  $ApBq$ , be  $d$ ; and  $\delta$  the decrement of this last attraction, when the oblate spheroid is diminished into the prolate, whose poles are  $AB$ : then, I say,  $d$  is nearly equal to  $\delta$ ; the difference of the axes of the generating ellipse being small.

For the attractive matter, that is taken away, has, in both cases, the same ratio to the matter, that is left; and its position, with respect to that which is left, is, in both cases, nearly the same: And therefore the successive attractions will be nearly in continued proportion,  $G : G - d :: G - d + \delta ::$ . Or multiplying and rejecting  $d^2$  as inconsiderable,  $Gd = G\delta$ , and  $d = \delta$ .

Thus, if the attractions of the sphere  $APBQ$ , and of the prolate spheroid, at its pole  $A$ , be 126 and 125 respectively; the attraction of the intermediate oblate spheroid at its equator will be  $125\frac{1}{2}$ : and how nearly this approaches to the truth, may be seen from an exact computation of those attractions. For, if the axes of the generating ellipse be 101 and 100, and the attractive force at the surface of the sphere 126; the attraction at the pole of the prolate spheroid will be 124.9838; and that at the equator of the oblate 125.5077; which exceeds the arithmetical mean

between



between the two former, only by .0068 ; that is, by about  $\frac{1}{18410}$ th part of the attraction of the oblate spheroid at the equator.

This reasoning is more shortly expressed (*Princip. lib. iii. Prop. 19.*) as follows :

.... “ Gravitas in loco *A* in sphæroidem, con-  
 “ volutione ellipsæos (*ApBq*) circa axem *AB* de-  
 “ scriptam, est ad gravitatem in eodem loco *A* in  
 “ sphæram centro *C* radio *AC* descriptam, ut 125  
 “ ad 126. Est autem gravitas in loco *A* in terram  
 “ media proportionalis inter gravitates in dictam  
 “ sphæroidem et sphæram ; propterea quod sphæra,  
 “ diminuendo diametrum *PQ* in ratione 101 ad 100,  
 “ vertitur in figuram terræ ; et hæc figura, diminu-  
 “ endo in eadem ratione diametrum tertiam, quæ dia-  
 “ metris *AP, PQ* perpendicularis est, vertitur in dic-  
 “ tam sphæroidem ; et gravitas in *A*, in *utroque casu*,  
 “ diminuitur in *eadem ratione* quam proxime.”

In which the expression “ *eadem ratione*” occurring a second time has misled F. Frisi and others, to think this last ratio to be likewise that of the axes, or of 101 to 100 : Whereas the *identity* of ratio’s here asserted is to be referred only to the words “ *utroque casu* ;” the ratio itself being not that of the axes, or of *m* to *n* ; but the half of that ratio (whatever it is found to be by *Prop. 91. lib. i.*) which the attraction of the sphere has to the polar attraction of the inscribed spheroid.

This inadvertence, however, of his own F. Frisi charges upon Sir Isaac Newton ; and files it up, as the sixth of the errors, which he says have been discovered in the *Principia*. . . . “ Ita dum stabilitæ  
 “ in 19 *lib. iii. propositione terrestrium axium pro-*  
 “ portionis

“ portionis fulcimentum et patrocinium quærimus,  
 “ aliud in propositione eadem *sophisma* sese offert,  
 “ quod eorum, quæ in principiis mathematicis New-  
 “ toni nacta (*i. e.* detecta) sunt hæctenus, sextum  
 “ est, &c.” But we may take it off the file again ;  
 and, for the present, leave the other five, till they are  
 considered of at more leisure.

13. In his 10th and last chapter, our author sums  
 up the evidence, and finds, that all the good observa-  
 tions, that have been made, as well by pendulums, as  
 by actual mensuration, concur with the theory, in  
 making the ratio of the earth's axis and equatorial  
 diameter to be as 229 to 230. This is indeed a suf-  
 ficient confirmation of the theory of gravitation : But  
 it must be observed, that the coincidence is not per-  
 haps quite so perfect as F. Frisi imagines. That  
 ratio corresponds well enough to the exactness, to  
 which the first elements of the calculus can be ob-  
 tained ; *the length of a second pendulum, and that of*  
*the earth's equatorial diameter*, from which the cen-  
 trifugal force ( $\frac{1}{289}$ ) is deduced. But, if we suppose  
 that force to be accurately  $\frac{1}{289}$ , and compute more  
 rigorously, we shall find the ratio in question to be  
 very nearly that of 225 to 226 ; agreeing still with  
 the observations as well as can be desired ; and shew-  
 ing, at the same time, the inimitable art of Sir Isaac  
 Newton in the contrivance and use of approxima-  
 tions ; seeing the strictest calculation raises the equa-  
 tor not the third part of a mean geographical mile  
 above what he had found by his method.

I sent F. Frisi's book to my ingenious and learned  
 friend the reverend Mr. Murdock, Fellow of this  
 Society, who has fully consider'd the question con-  
 cerning

cerning the figure of the earth; and who, after having perused the book, and discover'd the above mistake of F. Frisi, sent me the above theorem, and its demonstration. He likewise sent me the following theorems, which, he says, he had communicated to M. de Bremond, in the year 1740, when he was translating his treatise on sailing: But M. de Bremond dying soon after, those, who had the care of publishing the translation, printed it incorrect in several places; particularly the theorems for the prolate spheroid: On which account, he says, if they are thought worth preserving, they may be inserted in the *Philosophical Transactions*.

*Postscript.*

Theorems for computing the ratio of the attractive force of a spheroid, at its pole or equator, to that of the inscribed or circumscribed sphere.

1. In an oblate spheroid, the ratio is,

$$\begin{array}{l} \text{Pole} \\ \text{Equator} \end{array} \left\{ \left[ \begin{array}{l} 1 + \frac{1}{m^2 - 1} - \frac{m^2}{m^2 - 1} \times A : \frac{1}{3} \\ \frac{m^2}{m^2 - 1} \times A - \frac{1}{m^2 - 1} : \frac{2}{3} \end{array} \right] \right.$$

2. In a prolate spheroid, the ratio is,

$$\begin{array}{l} \text{Pole} \\ \text{Equator} \end{array} \left\{ \left[ \begin{array}{l} 1 - \frac{m^2}{m^2 - 1} + \frac{m}{m^2 - 1} \times l : \frac{1}{3} \\ \frac{m^2}{m^2 - 1} - \frac{m}{m^2 - 1} \times l : \frac{2}{3} \end{array} \right] \right.$$

In which  $m : 1$ , as the greater axis of the generating ellipse is to the lesser.  $A$  is a circular arc, to the radius 1, whose tangent is  $\sqrt{m^2 - 1}$ , or its reciprocal, if  $m^2 - 1 \ll 1$ . And  $l$  is the natural logarithm of  $\frac{S}{V}$ ,  $S$  being the sine of the arc, whose co-sine is  $\frac{1}{m} \times \sqrt{m^2 - 1}$ , and  $V$  the versed sine of the same arc.

*Note,* The two first theorems, by substituting  $t$  for  $\sqrt{m^2 - 1}$ , coincide with those of Mr. Maclaurin for the oblate spheroid, in his dissertation on the tides.

### III. *A Letter from the Rev. Mr. George Costard, Fellow of Wadham-College, Oxford, to Dr. Bevis, concerning the Year of the Eclipse foretold by Thales.*

Dear Sir,

Read Jan. 25, 1753. **I** THANK you greatly for the use of the Petersburg Acts, while in London, where Bayer supposeth the eclipse foretold by Thales to the Ionians fell out the year before Christ 603. Since my return home, upon looking over some papers, that I had formerly drawn up on that subject, I find, that I had determined it to have been the very same year. I will not trouble you with the chronological arguments, on which I founded that determination; and therefore shall only transcribe so much of those papers as relates to calculation.

Riccioli supposeth, that the eclipse foretold by Thales happened the year before Christ 585; and quotes both Theon and Cleomedes, in confirmation of the opinion. Theon, perhaps, had Cleomedes's words in view; but neither of these authors have circumstances enough to determine what eclipse in particular they meant. The passage of Theon is in his chapter concerning the moon's parallax, where he says, that Hipparchus, being in doubt whether the sun had any parallax at all, supposed, in the first book of his treatise concerning Magnitudes and Distances, that the earth, in respect of the sun, was only a point; from whence, by means of an eclipse there set down by him, he framed two distances of the sun, a less and a greater. *Εν γὰρ τῷ Α περὶ μεγεθῶν καὶ ἀποστημάτων λαμβάνει φαινόμενον τὸτο, Ἐκλείψην Ἥλιου, ἐν μὲν τοῖς περὶ τὸν Ἑλλησπόντον τόποις ἅλα τῆς Ἥλιος ἀκροβῶς γεγενημένην, ὥστε μηδὲν αὐτῆς παραφαίνεσθαι ἐν Αλεξανδρείᾳ. δὲ τῇ καὶ Αἰγυπτίῳ τὰ Δ μάλιστα πεμπτήμωρια τῆς διαμέτρου ἐκλελοιπότα.*

All then that is here said is, that the eclipse made use of by Hipparchus was total at the Hellespont; but at Alexandria in Egypt a little more than 5 digits only. But he hath neither given us the æra of Nabonassar, the place of the luminaries, nor any one circumstance besides, by which we might form any conclusion what year this eclipse was in.

Cleomedes, who perhaps saw the same treatise of Hipparchus, is as uncircumstantial as Theon. He says only, that the diameter of the moon's shadow at the earth is something more than 4000 stadia. And to confirm this assertion, he says, *γεγόνε δὲ καὶ αὐτῇ τήρησις ἐπὶ τῆς καλῆς τὸν Ἥλιον Ἐκλείψεως, ὅς ὅλος ΠΟΤΕ*

ἐν Ἑλλάσποντῳ ἐκλείπων, ἐτηρήθη ἐν Ἀλεξανδρείᾳ παρὰ τὸ πέμπτον τῆς ἰδίας ἐκλείπων διαμέτρου ὕπερ ἑστὶ καὶ τὴν φανίαν παρὰ δακτύλους δύο καὶ βραχύ.

By the quantity of obscuration here mentioned, this seems to have been the same eclipse with that quoted by Theon from Hipparchus; but as the place of observation in both these authors appears to have been Alexandria in Egypt, it must have been after that place was built. Consequently it was probably observed there by Hipparchus himself, and therefore could not have been the eclipse foretold by Thales. Besides, was this eclipse total upon the banks of the Hellespont, I know not what reason there is for supposing, that the battle between the Lydians and the Medes was fought there. It should rather seem, that the engagement was on the confines of the two kingdoms: consequently in a more southern latitude, and in a longitude more to the east of Alexandria, this eclipse could not have been total; nor therefore (as Herodotus said it did) turn day into night.

Sir Isaac Newton, in his chronology, likewise supposes the eclipse meant to have been that in May, the year before Christ 585. But in this, perhaps, he rather followed others, than adopted it after any examination of his own. That treatise never had the finishing hand of its great author, and it is well known now in what manner it came abroad.

According to Riccioli, this eclipse was at the Hellespont central, and at Sardes fell out at 6 of the clock in the afternoon; and therefore is rejected, you find, by Maier, in the Petersburg Acts, as being too late in the day. “ Quia ad littora Asiæ minoris

“(says he) sub folis occasum appulit, in Natolia  
 “ nulla fuit eclipsis totalis, quin nec conspicua ob  
 “ instantem folis occasum.”

According to my computation, the apparent time  
 of the true conjunction was at Greenwich, May 28,  
 4<sup>h</sup> 35' 15".

	s	o	'	"
The place of the luminaries	1	29	0	24
Moon's latitude north			20	54
Semidiameter of the earth's disk			61	14
Semidiameter of the penumbra			32	40

	d	h	'	"
Time of the greatest obscuration at Greenwich	28	4	32	33
Beginning of the general eclipse		2	3	20
End of the general eclipse		7	1	46
Duration.		4	58	26

	s	o	'	"
Longitude of Sardes east from London			29	5
Its longitude north			38	10
Time of sun-set there May 28, 7 <sup>h</sup> 6' 54".				

The passage of the centre of the penumbra over  
 the earth's disk was at the following times, as rec-  
 koned at Greenwich, thus:

Times			Longitudes			Latitudes		
n	i	ii	Q	i	ii	o	i	ii
3	22	33	115	5	10W	22	10	40 N
4	02	33	95	24	50	31	30	10
4	32	33	75	52	30	38	49	10
4	35	55	73	34	40	39	31	10
4	43	34	68	8	0	41	2	23
5	02	33	52	22	10	44	11	30
5	32	33	25	20	20	46	36	30

By this, I think, it pretty plainly appears, that the centre of the shadow passed so far from any place, where we can reasonably suppose the battle between the Lydians and Medes to have been fought, that this can hardly have been the eclipse mentioned by Herodotus.

Father Hardouin, in his chronology of the Old Testament, rejects this eclipse, as not happening, he says, in the reign of Cyaxares, but in that of Astyages; not on the fourth year of the Olympiad, but a month before it began; as falling out too late in the day; the greatest obscuration being (at Sardes, I suppose he means) scarce half an hour before sun-set; and not total, or central, or 12.56' digits, as Riccioli makes it, but almost 9. Tho' Pliny therefore says this eclipse was Olymp. XLVIII. 4. & A. V. C. CLX. yet six MSS. he observes, in the French king's library, have CLXX. and so most printed copies. He thinks therefore, that, instead of CLXX the number should be CLVII. which, he says, is Olympiad XLVIII. 4. and the year before Christ 597; when there was an eclipse of the sun, on Wednesday July the 9, at 6 of the clock in the morning.

This



This eclipse Petavius likewise prefers; tho' he makes the digits eclipsed only 9.22': Which is strange enough, as it could not have been by any means the cause of such a darkness, as is described by Herodotus. But F. Hardouin supposeth, that this battle was fought upon the banks of the river Halys in Cappadocia, and in latitude north  $40^{\circ}$ ; where, says he, this eclipse must have been central and annular.

According to Dr. Halley's tables, the year before Christ 597 the apparent time of the true conjunction at Greenwich was July 8<sup>d</sup> 21<sup>h</sup> 50' 9"; and

	s	o	'	"
The place of the luminaries	3	9	17	0
Moon's latitude north			33	32
Semidiameter of the earth's disk			54	38
Semidiameter of the penumbra			30	44
	d	h	'	"
Beginning of the general eclipse		19	8	16
End of the general eclipse	9	0	49	2
Duration		5	41	6
	d	'	"	
Sun rose centrally eclipsed, in longitude west from Greenwich		83	33	0
And in latitude north		43	26	50
Sun set centrally eclipsed, in longitude east from Greenwich		132	37	20
And in latitude north		26	25	20

The place of the centre of the penumbra was at the following times, as reckoned at Greenwich, thus:

Times

Times			Longitudes			Latitudes		
h	'	"	°	'	"	°	'	"
21	46	15	30	21	0 E	62	30	34 N
21	58	39	42	53	30	60	53	0
22	13	39	56	32	40	58	8	20
22	28	39	68	58	0	54	41	40

This eclipse, therefore, at Sardes, or any-where else that we can well suppose this battle to have been fought, could not have been great enough to turn day into night; and therefore doth not answer Herodotus's description.

Archbishop Usher rejects both these eclipses, as inconsistent with his chronology; and supposes that intended to have been A. M. 4113. An. Nab. 147. before Christ 601. Olymp. XLIV. 4. Sunday July 20. 3<sup>h</sup> 25'. before noon, digits eclipsed 9. But this likewise is greatly defective as to quantity.

But tho' this is insufficient for the purpose, yet there was one two years before this, or the year before Christ 603, that will be found by good tables intirely satisfactory. Petavius, indeed, makes the digits eclipsed only 7.20'; but, according to Dr. Halley's tables, the apparent time of the true conjunction was at Greenwich, May 17<sup>d</sup> 20<sup>h</sup> 42' 19". The place of the luminaries 15. 19° 12'. and the moon's latitude north 25' 17".

Semidiameter of the sun	15 49
Semidiameter of the moon	16 45
Semidiameter of the penumbra	32 34
Semidiameter of the earth's disk	60 50
	Beginning

	h	'	"
Beginning of the central eclipse	19	13	27
End of the central eclipse	22	3	47
Duration	2	50	20
Sun rose centrally eclipsed, in longitude west from Greenwich	43	46	10
And in latitude north	2	54	0
Sun set centrally eclipsed, in longitude east from Greenwich	155	56	20
And in latitude north	42	38	10

The place of the centre was at the following times, as reckoned there, thus :

Times			Longitudes			Latitudes		
h	'	"	o	'	"	o	'	"
19	13	27	43	46	10 W	2	54	0 N
20	8	37	19	19	30 E	31	33	0
20	23	37	28	28	10 E	35	47	50
20	42	19	40	29	30 E	40	41	20
22	3	47	155	56	20 E	42	38	10

By this it appears, that, if modern maps and geographers may be depended upon, the center of the shadow passed over the kingdom of Barca and Africa, and crossed the Mediterranean between Candia and Cyprus, and then over Antiochetta and Erzroum, and a little to the south of Kars.

You will see, Sir, how this agrees with what is said in the Petersburg Acts, pag. 332. which, therefore, I shall not transcribe. I shall only add, that, if any allowance is to be made for the moon's acceleration,

for the moon's acceleration, or any other cause, the track here given, as you know, will be a little different. As I cannot make several ancient eclipses, that I have tried, succeed to my mind, without some such supposition, I have done the same with regard to this. What the quantity to be allowed is, I leave to you and others to determine: At present I make it 45'; at Mr. Whiston's rate of 1' in 54 years, or thereabouts. Then

The apparent time of the true conjunction at Greenwich	17 20 42 19
For the moon's acceleration	45 0
Apparent time of the moon's conjunction at Greenwich corrected	17 19 57 19

From hence I find the passage of the penumbra at the following times as reckon'd at Greenwich, thus:

Times			Longitudes			Latitudes		
h	m	s	°	'	"	°	'	"
19	23	37	30	49	30 E	31	33	0 N
19	38	37	39	58	10	35	47	50
19	57	19	60	30	0	40	41	20

By this table it appears, that the center passed more to the south than the former, and went near Tripoli, Arafta, Nisabin, and Ardbil.

It is much to be wish'd, that Herodotus had told us where this battle was fought; that, by this means, we might have known, which of these two paths to have preferred. However, as he hath not, and there

is nothing in either of them, that is inconsistent with the history, I conclude, from a number of other circumstances besides, that this really was the eclipse foretold by Thales. I was not a little pleased, upon looking into my papers, to find, that Bayer and I agreed so exactly in the very year, as I was a stranger to what he had said upon that subject, till you brought me that volume of the *Petersburgh Acts*; and as the principles I proceeded upon were something different from his. This, however, is a presumption, that we are right, and confirms my opinion of some other articles in chronology, wherein I differ from some very great names. I am,

Dear Sir,

Ulip, May 21,  
1752.

Your very faithful and

obliged humble servant,

G. Costard.

IV. *An Account of the Case of Anne Elizabeth Queriot, of Paris, whose Bones were distorted and softened; by Ambrose Hofty, M. D. of the Faculty of Paris.*

Read Jan. 25.  
1753. **A**NNE Elizabeth Queriot\*, aged 35, native of Paris, was married in the year 1746 to a wool-carder. Tho' seemingly of

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\* Her marriage-name was Supiot.





J. Haycock del.

# An extraordinary Case of the successive softening of the Bones.

- a. a. The Balls of the Knees
- b. b. The Condyles or Elbows
- c. The right Wrist ulnarly in-
- d. The force Deformity which appeared
- e. e. The Clavicles raised into an arch
- f. The point of the sternum raised
- f. toward obliquely towards the right
- g. Swelling on the second of the ribs
- h. h. The portions of the Breast dysphyl before
- i. i. The Bones bent within
- k. k. Parts of the Skin become callous.





of a delicate constitution, yet she enjoyed tolerable health, having never had any dangerous illness before her marriage. She was brought to-bed in 1747, in the month of September, and for the first time complained of great weakness in the small of her back, loins, and thighs, and could scarce walk.

A second lying-in, a year after, removed her complaints for about six weeks; after which they returned. In the year 1749, being two months and a half with child, she was unaccountably seized with a loss of blood; upon which she miscarried.

Two months after, she fell accidentally upon her left side; which gave her great pain in the leg, thigh, and hip of that side, and made them swell: but there was neither fracture or dislocation.

Her pains in some time abated; but the weakness of her limbs continued. She was soon after with child, and, in the beginning of her pregnancy, had a second fall; which revived her former pains, and caused new ones all over her body, and a swelling, as before. This confined her to her bed, yet her pregnancy went on very well, and terminated as happily; by which the swelling went off; but her limbs were so weak, that she could not bear upon her feet.

She then fell into the hands of quacks, who gave nothing worth mentioning, that I could hear of. About a year since she was blooded on the hand twice, took a course of baths in the decoction of the ashes of vine-twigs, had different pultices applied to her, and embrocations of oils and balsams, which enabled her to walk by the help of crutches;

In about six months after child-birth, her pains returned more violent than before; and about the

same time, an abundance of white chalky sediment appear'd in her urine; and the fore-finger of her right-hand was observed to be distorted towards the little finger; which was the first appearance of the dissolution, that ensued. Soon after the lower extremities began to turn upwards gradually, and almost in a parallel line with her body, and continuing, in nine months was reduced to the posture exhibited by the print drawn last August. All the bones were affected, especially the thorax, which had lost its natural form and capacity.

This miserable state was constantly attended with exquisite pains; and, according to the seat thereof, the patient used to say, "Now such a part works." Sometimes they abated, and then she felt so sore, as not to bear being touched: And during this ease from her pains, a quantity of the above-said sediment pass'd by urine, and little or none in her sufferings. It is quite cretaceous, and, reduced into a fine powder, ferments gently with acids. Little of it appeared since I began to visit the patient; so that I could not get a sufficient quantity thereof to make farther inquiry. I refer to the print; it being almost impossible, without it, to conceive the figure she made, and the distortion of her limbs.

She could bear no covering, but a few napkins, both from inward heat, and to avoid loading her breast. I shall only remark, that, since the print was drawn, her right foot and hand got above her head. Notwithstanding this preternatural posture, the evacuations by stool and urine were regularly performed, and with great ease. Her flesh seemed dead and cedematous, the skin rough and scaly,

scaly; so that I often apprehended a mortification. She had a cough, a laborious respiration, and sometimes a spitting of blood, from the coarctation of her breast, all its bones plying inwardly. She was capable of no other motion but of turning her head on both sides, stirring her left arm in the shoulder-joint only, and separating her fingers, but not bending them. She had her menses regularly, till about three months before her death. She generally had a low fever, inward heat, sweats, and restlessness. She took antiscorbutics during the months of June and July, to no purpose. Her fever ran very high in August, attended with deliriums, headachs, ravings, and *subfultus tendinum*.

A little before her death came on a deafness, a dimness of sight, a scalding of her eyes, and a constant dropping; violent pains in her head; in short, a great weakness in all the organs, which shew'd how much the head was then affected.

The distortion of her limbs went on so fast in August and September, that almost every third day I could observe something new; especially the left foot, during that time, came down gradually near 18 inches from under her ear, where it lay before. It was also observed in August, that her neck grew visibly smaller, the thorax much narrower. I then also remarked, that the napkins, upon which she spit, grew black in the washing, and stained as from the mercurial ointment; tho' I could not suspect it, as I could not learn she had ever used any mercury. In a month after, I observed the same thing on all the linen, that touched her skin. I got a napkin rubbed with soap, then dried, and afterwards washed. This  
method

method had almost taken off the stains, as it does those from the mercurial ointment.

Her linen stained all the washing, like linen impregnated with the above-said unguent. Those spots appeared on the linen a mixture of a cretaceous matter and grease.

Since this remark was made, none of the white sediment was seen. This, and the apparent nature of the stains, made me believe, that it was then discharged by spittle, and the pores of the skin, and mixed with oily particles of her fluids, which had acquired a quality analogous (if I may so speak) to that of mercury, of staining all linen. I am also apt to think, that this sediment was the earthy matter, that gives the bones their solidity and hardness, which had been dissolved by the same vitiated quality of the fluids, and evacuated by the emunctories already mentioned.

After great sufferings, she died the 9th of November. By the consent of her husband, I had her open'd, in the presence of some of the most celebrated anatomists and academicians of this city. The operation was begun on the left tibia, cutting on the fore-part of it, from below the knee to its basis. It was wonderfully alter'd; more or less soft in all its length; in some points intirely dissolved, and its sides not thicker than the gristle of the ear. The spongy substance of its extremities supple, yielding to the least pressure. The reticular matter was quite destroyed. The peroné was intirely dissolved in the middle, and only slight marks of its extremities remain'd. Instead of marrow, we found in all the bones a red thick matter, like coagulated blood  
mixed

mixed with grease. The rotula was intire, but very soft and spongy; the condyles of the femur the same. All the cartilages were found in their natural state. The head of the humerus was much diminished and flatten'd: Its middle part very small, pliable, softened in all points, yet in some friable. The cubitus and radius suffer'd the same alterations with the humerus. By stretching all her limbs we laid them streight; but they soon after returned to their former curve. The phalanges of the fingers were not so much soften'd, but were easily cut, and bent like whale-bone. The femur was rather a fleshy body than a bone; its cavity was filled with a reddish suet, instead of marrow, which, accumulated in different points, bulged out the fleshy sides. The capacity of the pelvis was much diminished; the bones, that compose it, were soften'd, thicken'd, and contracted. The spine kept its natural form; the vertebræ soft and supple. The sternum, and all the cellular bones, seem'd solid, but could bend, and were easily cut. The ribs, tho' soften'd, were still friable. Some of them, towards the sternum, were doubled over one another. The clavicles seem'd almost cartilaginous. The shoulder-blades were much thicker than natural, less broad, and intirely disfigured. The two protuberances call'd acromion and coracoides almost joined. The scull-bones were easily cut in slices, twice as thick as in their natural state. Both plates were joined in one, and no traces at all of a diploe. Their substance abounded with an extremely diluted serum, easily squeezed out by a gentle pressure of one's fingers. The futures almost obliterated: The bones of the basis and face shared in the calamity. The teeth  
hard

hard as usual. The dura mater was incorporated with the bones. The brain not softer than ordinary: Its right hemisphere was by one third larger than the left; and hence, perhaps, the weakness of her left side, often manifested by pains, aches, defluxions, heaviness, falls on that side, and every illness, which she had from her infancy, beginning in some part thereof. When young, she fell upon her head down two pair of stairs. The membranes, that separate the two hemispheres of the brain, were much thicker than commonly.

In fine, all her bones were so soft, that the scalpel with very little force ran thro' the hardest of them; even the rocky apophyse of the ear bone, so called from its excessive hardness. Nothing extraordinary was found in the viscera; but their size diminished by the compression, and an universal cachexy.

There could be no cause assigned of this woman's disorder, as she gave no signs plain enough to prove either a scurvy, pox, or king's-evil, either hereditary, or acquired; her parents having lived healthy, the one to the age of eighty, and her mother being still alive, aged sixty, and in good health. She had three children, who died of disorders common to their age. One, 4 years old, died of the measles.

The case was looked upon as very extraordinary, and the patient was seen as a curiosity. It is certainly rare, but there have been some similar cases, which are cited in the *Abridgment of the Philosophical Transactions*, in the remark upon the like case presented to the Royal Society, by Mr. Silvanus Bevan,

Bevan, p. 458, &c. Vol. II. This differs from the other examples, by the sediment of the urine, the stain on her linen, the preternatural situation of her limbs. Something very singular was, that she did not blow her nose perhaps once a month, even in her health; always slept with her mouth open'd, and her tongue hanging out. The manner in which such dissolutions of bones are accounted for, in the above-mention'd remark, seems the most rational, plain, and satisfactory, that can be given.

I attended this patient since last August with Dr. Morand, and kept a journal of our observations, hoping it may be of use to the public, and worth the notice of the curious.

Paris, Dec. 10,  
1752.

Hofsy, M. D.

V. *An account of a Roman altar, with an inscription upon it, found in April last at York, and communicated to the Society of Antiquaries by Mr. Francis Drake, F.R.S. As also a breif explication of the inscription by John Ward, LL.D. Professor of Rhetoric in Gresham College, and V. P. R. S.*

Read Feb. 1, 1753. **T**HIS altar was found, with other remains of antiquity, by some workmen, in opening a new and deep drain down the centre of a large street, called *Micklegate*, in the  
E city

city of *York*. Its height with the pedestal, on which it stands, and which is made hollow to receive it, is fourteen inches and a quarter. But the breadth varies in several parts of it, according to their different form. On the top is an apex, with a volute on each side, and on the front a pediment over the inscription. It is elegant for the workmanship, and well preserved. And that a better judgement may be formed of it at this distance, Mr. *Drake* has sent up a draught of it in its just proportion, with the inscription upon it; as also another copy of the inscription, taken off from the stone, by pressing wet paper into the letters, and then delineating both them and the stops with a pencil. A copy of the draught, but reduced to one half of the size, accompanies this discourse, to which I take leave to refer (1); and shall only subjoin here the inscription itself in words at length, as I think it may be read.

*Matribus Africis, Italicis, Germanicis, (2).*  
*Marcus Minucius Ande.*  
*miles legionis sextae victricis,*  
*gubernator legionis sextae,*  
*votum solvit libentissime merito.*

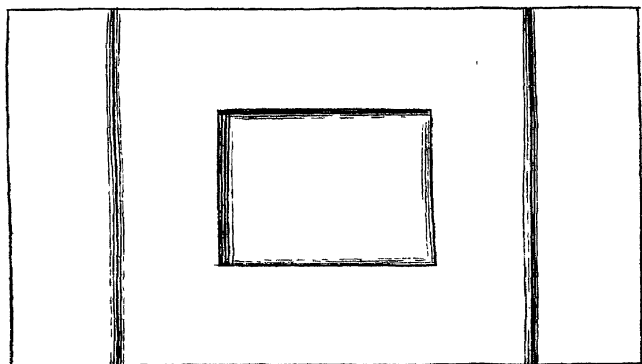
I. By

(1) See TAB. I. Fig. 2.

(2) The abbreviation of this word being G N. as Mr. *Drake* affords me in a letter, which I received from him, since I wrote this discourse; it may, I presume, be read, as I have now given it. Tho, in the copy of the inscription delineated upon the stone, the vestiges of the letters seemed to be G A L. for which reason I then read it *Gallicis*.







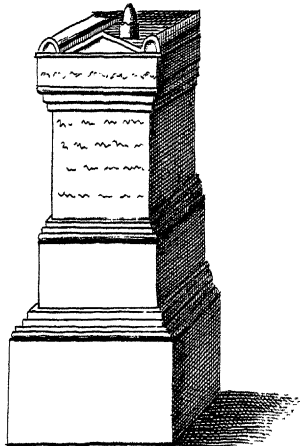
*Fig 3 p 333.*



Fig. 2. p. 124.



Fig. 1. p. 34.



A perspective View of  
Fig. 1.



1. By this inscription, and many others of the like sort, found in *Britain* and other countries, it appears, that these female deities, under the name of *Matres*, were worshiped in most parts of the Roman territories. So that the observation of the learned *Spon* seems not to have been so well founded, when he says: *Notandum, has Matres fere tantum in Gallia et Germania fuisse cultas, raro enim alibi earum reperiuntur inscriptae arae* (1). In most of these inscriptions they are only called *Matres*, but in some *Deae Matres*; and frequently the country, or particular place, where they were worshiped, is subjoined. Thus, in *Horfley's Britannia Romana*, among the inscriptions found in *Britain*, we have, *Matres domesticae, Matres Alatervae, et Matres campestris*; as also *Matres tramarinae*, for *transmarinae* (2). Which last title, being in another inscription written *Matres tramai* (3), occasioned the learned *Selden* to say: *Quid sibi vellet, ne bariolari quidem ausus sum* (4). And in the inscription now under consideration we meet with *Matres Africae, Italicae, Germanicae*.

Writers are not agreed in their opinion concerning these female deities, who were worshiped under the character of *Matres*. *Spon* supposes they were deified women, who, while living, were thought to have the gift of prophecy. The reasons for which opinion having been given at large in *Horfley*, I shall for brevity sake take leave to refer to the passage (5).

E 2

But

(1) *Miscel. erud. antiq.* p. 105.

(2) *Brit. Rom. Cumb.* XL. *Scotl.* XXIX. *Cumb.* LI.

(3) *Ibid.* p. 298.

(4) *De Diis Syris, Syntagm.* II. c. 2.

(5) *Pag.* 201. See also *Rsinef. Cl.* I. n. 175. p. 188.

But *Selden* applies them all to the *Dea Syria*, or *Mater Deorum*; whom *St. Augustin*, as he observes, takes for *Juno*, and saith: *Tot esse Junones, quot sunt simulacra* (1). Agreeably to which notion we find several altars in *Gruter* inscribed *Junonibus*, in the plural number (2). And *Plutarch* takes notice of the worship paid to the *Deum Matri* at *Enguium* in *Sicily* (3); which *Cicero* seems to allude to, when he saith, *Matris magnae fanum apud Enguinos est* (4). I shall only add further upon this head, that, as we meet with several inscriptions, which have on them the title of *Matronae*, to whom they are addressed; *Spon* thinks, not improbably, that these *Matronae* were the same deities as the *Matres*, or *Matrae*, as they are sometimes written, who were indifferently worshiped by each of those titles, of which he has given a variety of instances (5).

2. The two first names of the person, who dedicated this altar, were doubtless *Marcus Minucius*; but the third, as here abbreviated, is uncertain. Tho I apprehend, that the two first letters in the ligature may be AN. which, with the two following DE. may stand for *Andegavus*, or, as it is sometimes written, *Andecavus*; as denoting the name of his country, now called *Anjou* in *France*.

3. This

(1) *Ubi supra*.

(2) *Pag. xxiv. 1, 2, 3, 4, et alibi.*

(3) *In vit. Marcelli.*

(4) *In Verr. lib. iv. c. 44.*

(5) *Ubi supra, pag. 104.*

3. This *Marcus Minucius* describes himself by two characters or employments; first, as a *soldier of the sixth legion*, which was honored with the title of *viatrix*; and then as *pilot of the same legion*, the epithet *viatrix* not being repeated the second time, as unnecessary. It was not unusual among the *Romans* for persons to express their several titles, or employments of life, in the inscriptions cut upon such votive altars. Many of these are to be met with in collections of this kind; in some of which they are connected by the particle *et*; and in others not, as in the instance before us. I shall produce an example or two of each kind, to set this matter in a clearer light. Of the former sort we have in *Gruter*:

I. O. M.  
ET. FORTVNAE. SECVNDAE  
CAES. M. ANTONINI. IMP  
M. LICINIVS. COR. II. VIR  
COL. DAC. SARMIZ. ET. FLA  
MEN. DIALIS. SIGNVM. IO  
VIS. EX. VETVSTATE. RE  
STITVIT (1).

And again:

APOLLINI. AVG  
SACR  
FELIX. AVG. LIB. OPTIO  
ET. EXACTOR. AVRI. ET  
ARGENTII. ET. AERIS (2).

In

(1) *Pag. VIII. I.*

(2) *Pag. XXXVI. 8.*

In both these instances the conjunction *ET* is expressed between the two characters, but in others it is omitted. As in the same author :

HERCVLI INVICTO  
M. CASSIVS HOR  
TENSIVS  
PAVLINVS PRAE. VRB.  
XV. VIR  
SACRIS FACIVND.  
DONVM DEDIT (1)

So also in *Reinesius*:

IVNONI. REGIN  
SACRVM  
D. N. DIOCLETIANI IN  
CLYTI. PRINCIP. SEM. AVG.  
PINIVS PAVLLVS SILVANVS  
VIR CLAR. PRAEF. VRB.  
IVDEX. SACRARVM. COGNITIO  
NVM. VOLVNT. EIVS CVR. (2).

And the like we meet with in *Horsley*, where one *Marcus Censorius Cornelianus* styles himself, *Centurio legionis decimae Fretensis, praefectus cohortis primae Hispanorum* (3). In the first four of these instances the different characters given to the several persons are such, as might belong to them at the same time ; but in the last they seem not compatible.

4. The

- (1) *Pag. XLVIII. ult. in tab. priori.*  
(2) *Classf. I. num. 282, p. 241.*  
(3) *Cumberl. LXIII.*



4. The title *gubernator*, or *pilot*, I do not remember to have met with in any other Roman inscription. And notwithstanding the first line is placed at some distance from the rest; yet it may, I think, connect with them, without supplying the word *sacrum*, in the following manner: *Matribus Africis, Italicis, Germanicis, Marcus Minucius*, etc. *votum solvit libentissime merito*. An instance of the like kind may be seen in *Horsley*, ending with the same letters, *VS LLM*, which he reads in the same manner (1).

It appears, by an inscription in *Gruter* (2), and republished by *Dr. Gale* (3), that this legion was transported from *Germany* to *Britain* in the reign of the emperor *Hadrian*, under the command of *Marcus Pontius*; who is there called, *Tribunus militum legionis sextae victricis, cum qua ex Germania in Britanniam transit*. The inscription therefore upon this altar at *York* may, as I apprehend, refer to that voyage; and intimate to us, that *Marcus Minucius*, by whom it was erected, was then pilot to the legion. It is probable indeed, as *Horsley* observes (4),  
 \* that

(1) *Scotland* xxvii. *Reinesius*, *Class. 1. num. 3*, reads the letters *LL. laetus libens*, and cites for it an inscription in *Gruter*, pag. *MLXXXIV. 4*. which ends with the abbreviated words, *LAET. LIB. FECIT*. And there is another inscription published by *Spon*, in his *Miscell. erud. ant.* p. 107; and afterwards by *Fabretti*, pag. 690, n. *III.* which has the words *LAETVS. LIBENS.* at length. However *Reinesius* adds: *Vocabulum positivi geminatum infert superlativum*; so *BB. bene bene*, is the same as *optime*; and *LL.* may stand for *lubentissime* or *libentissime*. And in like manner *MM.* is read in the superlative *meritissimo*.

(2) *Pag. cccclvii. 2.*

(3) *Antonin. Iter Britann. pag. 47.*

(4) *Pag. 79.*

that upon its first arrival it made no stop in the south, but marched directly by the usual rout to *Hadrian's vallum*; since there are several inscriptions upon and near the wall, both in *Northumberland* and *Cumberland*, wherein this legion is mentioned. And in the following reign of *Antoninus Pius* we find, by several other inscriptions, that it was in *Scotland*, and had a share in building the wall there (1). Not long after it might very probably be stationed at *York*, where *Ptolemy* places it (2), who lived under the next emperor *Marcus Aurelius*, as we learn from *Suidas* (3). The legion therefore being thus settled, *Marcus Minucius* might then think it a proper time to pay his vows, formerly made to those deities, whom he addresses in the inscription cut upon this altar.

I thought it necessary to give this view of the matter, for the sake of obviating an objection, which possibly may be made, as well with regard to the time of erecting the altar, as to the person, by whom it was erected. What I more especially refer to, is the form of the letters G and L, as they appear upon the altar. For altho they are found in the like shape in several British inscriptions published by *Horsley*, yet none of those seem to be altogether so antient. There is one with the G, which that writer is uncertain, whether to refer to *Commodus* or *Caracalla* (4). And another with the L plainly relates to the reign of *Severus*, by the names  
of

(1) See *Horsley, Scotland*, IV, VII, XVIII.

(2) *Horsley*, pag. 359.

(3) *In voc.* Πτολεμαῖος, ὁ Κλαύδιος χερματίσας.

(4) *Lancashire*, III.

of the consuls *Aper* and *Maximus*, who are mentioned in it, and answer to the year 207 of our common aera (1). The rest of them have nothing certain to determine their age; tho by some circumstances which attend them, they seem generally to agree better with later times. But if that particular form of either of those letters is not to be found in any of our *British* inscriptions hitherto published, before the reign of *Commodus* or *Severus*; it by no means follows, that it might not have been used sooner. And there was only the reign of *Marcus Aurelius*, that intervened between those of *Antoninus Pius* and *Commodus*, who in a few months was succeeded by *Severus*. Besides, as the sixth legion, after it settled at *York*, seems to have been stately quartered there, till the *Romans* left *Britain* (2); I do not perceive, either to what other time, or person with the character of pilot to that legion, the erection of this altar can be ascribed. But this I leave to the further consideration of the curious, to judge of it as they please.

G. C. Nov. 9,  
1752.

J. Ward.

(1) *Cumberland*, XLIV.

(2) See *Horfley*, p. 79.

VI. *An Account of several Persons seized with the Goal-Fever, working in Newgate; and of the Manner, in which the Infection was communicated to one intiré Family; by John Pringle, M. D. F. R. S.*

Read Feb. 1, 1753 **H**AVING lately had an opportunity of seeing several cases of the true goal-fever arising from the gaol itself, I thought it would not be improper to lay before the Society a short account of the manner, in which those persons were seized, the chief symptoms and progress of the disease, with some remarks upon it, in order farther to illustrate what I have advanced elsewhere, concerning the danger arising from foul air, and the agreement of this distemper with what has been called the fever of the hospital, or more generally a malignant or pestilential fever.

In the month of October 1750, a committee of the court of aldermen was appointed to inquire into the best means for procuring in Newgate such a purity of air, as might prevent the rise of those infectious distempers, which not only had been destructive to the prisoners themselves, but dangerous to others, who had any communication with them; and particularly to the courts of justice upon the trial of malefactors; whereof a fatal instance had occurred that year at the sessions held in the Old-Bailey, when the lord mayor, two of the judges, and one of the aldermen upon the bench, with several other persons then present, were seized with a malignant fever, and died.

The

The reverend Dr. Hales and I being consulted by the committee upon the point referred to them, and having visited the gaol in company with those gentlemen, it was then agreed, that, considering the smallness of the place, in proportion to the number of the prisoners, it would be proper to make a farther trial of the ventilator, and to have it worked by a machine, in the manner of a windmill, to be erected for that purpose upon the leads of Newgate.

This scheme was laid before the court of aldermen, and approved of, but not put in execution till near two years after. For on the 11 of July 1752, Dr. Hales acquainted Dr. Knight and me, that several of the tubes were finished, and that the machine had been going about six weeks; wherefore, being desirous to see the effects, he had appointed Mr. Stibbs the carpenter, employed in that work, to meet him that day at Newgate, and desired us to go along with. We went accordingly, and having visited several of the wards, we were all of us very sensible, that such as were provided with ventilating tubes were much less offensive than the rest that wanted them; and Dr. Hales and I could perceive a considerable improvement made upon the air of the whole gaol, since the time we had been first there with the committee. Some of the wards were so free from any smell peculiar to such places, that I am persuaded, were Dr. Hales's design completed, and a person appointed to regulate the sliders of the tubes, and to keep the machine in order, the usual bad consequences from foul and crouded goals, might in a great measure, if not wholly, be prevented in Newgate.

One of the wards allotted for the women had a small room adjoining to it, in which they usually slept. Both places seem'd at that time well air'd, tho' the latter was close, and, if I mistake not, without either window or chimney. The prisoners informed us, that, before this ward received the tube, this sleeping-place had been very offensive, but that soon after it became sweet; and tho' upon the first working of the ventilator they had been more sickly than before, they soon recover'd their health, and had preserved it ever since. Now from this account we must not infer, that any danger will arise from a sudden change of bad air for good; since this accident may be better accounted for from another circumstance, which we were then likewise told of; *viz.* that this ward of the women had been supplied by a ventilating tube before those in the lower story, where the air being in a more corrupted state, it had passed from thence thro' the seams of the floor, and other passages, to replace that, which was drawn off by the tube in the ward above: but that after the bad air was exhausted, the benefit of the fresh air soon appeared, by the better health of the prisoners.

But as it was not my design in this paper to set forth all the advantages, that may be expected from the ventilator, I shall leave that subject to be treated of by the ingenious inventor of it; and shall only take notice, that the tubes from the several wards, uniting in one great trunk, convey all the putrid steams by that channel into the atmosphere, through a vent made for that purpose in the leads of Newgate; and that tho' the wind was moderate during the time we staid in the gaol, yet we observed, that the ventilator

lator threw out a considerable stream of air, of a most offensive smell.

Before we parted, Mr. Stibbs informed us, that Clayton Hand, one of his journeymen, whilst he was employed in setting up the tubes, was seized with a fever, and carried to St. Thomas's hospital, after lying some days ill at his own house. Whereupon, apprehending that this man's sickness might be owing to the air of the gaol, Dr. Knight and I having the curiosity a few days after to go to St. Thomas's to make the inquiry, we found the patient sitting in one of the courts, recovered of his fever, tho' still weak, and had the following account from himself:

He said, that upon first finding himself indisposed, he had left off work for some days; but upon growing better he had returned to Newgate. That soon after happening to open one of the tubes of the old ventilator, which had stood there for three or four years, such an offensive smell issued from it, that being immediately seized with a nausea and sickness at his stomach, he was obliged to go home, and that the night after he fell into a fever, in which he lay about eight days before his friends carried him to the hospital. That becoming soon delirious, he recollected no other symptom, succeeding these mentioned, besides frequent retchings to vomit, a trembling of his hands, and a constant head-ach. This man had taken no medicine before he came into St. Thomas's, and since that time was attended by Dr. Reeves; but, as that gentleman was not then present, we were informed by the apothecary, that Clayton Hand had been admitted in the advanced state of a continued fever,

fever, attended with a stupor, and a sunk pulse, and that the fever had not left him till several days after his admission. The nurse's account was, that he had all along lain like one stupified, and that after the fever went off, he had continued for some time very dull of hearing. We could learn nothing certain about the precise duration of the fever, but from what the patient and his attendants told us, we collected, that he must have been ill between two and three weeks. So that from all these marks we made little doubt, but that this person had been ill of the true gaol-distemper; and were confirmed in our opinion by the following circumstance.

In company with the convalescent was one Thomas Wilmot, another of Mr. Stibbs's journeymen, who had likewise worked in Newgate, and whom we remembered a few days before to have seen in that place, very active, and in perfect health. This man told us, he had come to see his companion, but as he apprehended himself in danger of falling into the same fever, he should therefore be glad of our advice. Upon examination we found his tongue white, his pulse quick, and that he complained of a pain and confusion of his head, with a shaking of his hands, and a weakness in his limbs. He said his disorder had come on gradually, since the time we saw him in Newgate, but that he was then so very ill, he could work no longer. From which account it appeared to us, that this man had also caught the infection; but as the fever seemed not to be quite formed, we had hopes of stopping its progress: and with this view we advised him to take a vomit, and on the following night a sudorific. He followed the prescription,



prescription, and the effects shall afterwards be mentioned.

After Wilmot had told us his own case, he informed us of the indisposition of three more of his companions, who had been likewise employed by Mr. Stibbs in Newgate : whereupon we took their direction, visited them, and found them all ill of the gaol-distemper.

The first was Michael Sewel, who lodged in the Swan-yard near Newgate. This man had been ten days confined to his bed, without taking any medicine. He was then delirious, and had the petechial eruption : but observing, that he lay in a close, ill-air'd, and dirty room, without any attendants but his wife, then suckling a child, we believed he had no chance to recover where he was, and therefore recommended his case to Mr. Stibbs, who procured his admission that day into St. Thomas's hospital ; where he also recover'd.

The second was Adam Chaddocks, who lay at a green-shop in the little Old Bailey. He was taken ill on the same day with the former, and had used no medicine. He had likewise the petechial spots upon his breast and back ; and tho' he was not altogether insensible, was affected with a stupor attended with a sunk pulse, and other symptoms of the distemper. His landlady, who took care of him, informed us, that he had been troubled with retchings to vomit, and an head-ach from the beginning, and that for some days past he had been seized with a looseness, and that his stools were very offensive. As the room this person lay in was large and well-air'd, we did not think it necessary to remove him, but recommended

recommended him to the care of Dr. Pate, physician of St. Bartholomew's hospital, who attended him till he recover'd.

The third was John Dobie, apprentice to Mr. Stibbs, a lad about 15 years of age, who liv'd with his parents in a court by the White Bear in Canon-street. We saw him on the same day with the other two, which was the fourteenth of his sickness, and the twelfth since he took to his bed. His mother told us, that some of the journeymen working in Newgate had forced him to go down into the great trunk of the ventilator, in order to bring up a wig, which one of them had thrown into it; and that, as the machine was then working, he had almost been suffocated with the stench, before they could get him up. That upon coming home he complained of a violent head-ach, a great disorder in his stomach, with retchings to vomit, which had never intirely left him. We found him extremely low, with a sunk pulse, a delirium, and an unusual anxiety or oppression about his breast. This last symptom we ascribed to the opiates he was then taking for a looseness, which had come on two or three days before we saw him. He being in no condition to be moved, and being besides well attended by his mother, and in a well-aired chamber, we prescribed to him there, and repeated our visits, till he was quite free of the fever. It was observable, that before he was taken ill, he had been twice let down into the great trunk of the ventilator, when the machine was standing still, without complaining of any ill smell, or receiving any hurt thereby; but that the last time, when the machine was working, he immediately cried out he was ready to  
be

be suffocated ; and the two men who helped him out, by receiving the foul steam from the trunk, were both set a vomiting so violently as to bring up blood.

On the 23 of August, Thomas Wilmot, above-mentioned, called upon Dr. Knight, and told him, that, after taking the vomit and sudorific, he had immediately recovered ; but begged him to see his wife, who then lay ill of a fever, at his house in Snow's-fields, Southwark. The doctor suspecting, that this woman's indisposition might be owing to the contagion received from her husband, acquainted me with it, and carried me to see her. There we were informed, that Wilmot's daughter, a girl of eight years old, who lay with her parents, had been seized with a fever, soon after her father's recovery ; that she had been ill about a fortnight, and they believed had spots upon her breast, but that she had recovered without any medicine. That her mother had not only nurs'd her, but continued to lie with her ; and that some time after the girl's recovery, the mother began to complain, and soon after fell into a fever ; and that it was the twelfth day since she was confined to her bed. This woman having the *petechiæ*, a stupor, with deafness, and a sunk pulse, there was no doubt of her being likewise infected with the distemper, and probably by her daughter. As she had been without any assistance, we advised her husband to send for Mr. Breach, apothecary, in the Borough, who having served in the hospital of the army during the war, was well acquainted with the nature of such fevers ; and having left directions with him, we did not return till after the crisis ; which happen'd upon the 16 or 17 day from the time she was confined to her bed.

Some time after this, Mr. Breach the apothecary informed us, that he was again employ'd in Thomas Wilmot's family; for that Elizabeth Marshall, his sister-in-law, after nursing his wife, was taken ill of the same kind of fever, and desired our assistance. This person we found in the same bed, and in the same condition, in which we had seen her sister some time before; and in the room with her, in another bed, a son of Wilmot's, a boy of nine years old, ill of the same distemper. The former had been attack'd on the 15 of September, and the latter the day before. The woman's fever ran out the ordinary length of 16 or 17 days, but the boy's came some days sooner to a crisis, and was all along of a milder nature. She recover'd very slowly, complaining of great weakness, deafness, and a confusion in her head, the ordinary consequence of these malignant fevers.

One day, in my return from this house, I call'd at St. Thomas's hospital, to inquire for one William Thomson, a lad of about 16 years of age, who, as Wilmot then told me, was another of Mr. Stibbs's journeymen, and had been taken ill by working in Newgate, since the three he had mention'd before. This lad was recover'd, but not yet dismiss'd. He said, that upon finding himself growing ill, he had left his work, and kept at home for about a week, complaining of a pain in the hinder part of his head, and in his back, of a trembling of his hands, and of restless nights; that his feverish indisposition increasing, he had been obliged to take to his bed, where he lay about eight days before he was sent to the hospital. The apothecary added, that he had continued under their care about the same number of days before  
the

the turn of his fever; that his pulse had been extremely low all that time, and that they believed him to be in the utmost danger. He added, that the wife of Michael Sewel (the second patient they had received of those, who had been employ'd in Newgate) some days after her husband's admission, came to seek advice for herself, and that her complaints had been the same with Wilmot's at the time we saw him: he added, that he had given her some medicines, but had heard nothing of her since.

On the last day of December Mr. Breach informed me, that about a month before, he had been call'd to attend Thomas Wilmot; but as he died before he saw him, he could give no other account of his sickness, than as they told him; *viz.* that he had long been in a bad state of health, and that at last he became feverish, and went off with a looseness.

In the beginning of this month (January 1753) the widow applied to Dr. Hales and me, in order to have the distress of her family attested, and laid before the lord mayor, in hopes of having some provision made for them. Upon which occasion we learned, that Thomas Wilmot her husband, after taking the sudorific, so far recover'd as to work at his business; but that tho' he did not return to Newgate, yet his strength would not permit him to continue at work above a day or two at a time; still complaining of an head-ach, and pains across his breast, or, as he express'd it, about his heart, of a feebleness of his limbs, a shaking of his hands, and a constant drought. That notwithstanding these ailments he went daily, till a week before he died, when he grew very weak, and more feverish, had sometimes profuse sweats, and









Mens names.

Mr. Sewel	*	Mr. Morris
Mr. Hand	*	Mr. Bates
Mr. Willmot	* died,	Mr. Thompson *
Mr. Letts		Burton, apprentice
Mr Chaddock	*	Dobie, ditto *
Mr. Ruft	*	

*N. B.* All those mark'd \*, had the gaol-distemper.

*VII. An Account of the great Alterations which the Islands of Sylley have undergone since the Time of the Antients, who mention them, as to their Number, Extent, and Position: In a Letter to the Rev. Thomas Birch, D.D. Secr. R. S. by the Rev. Mr. Wm Borlase, M. A. F. R. S.*

Reverend Sir,

Read Feb. 8, 1753. **H**AVING made a little tour to the islands of Sylley last summer, and finding them very different from what I imagin'd, and from the descriptions given us of them, and the trade to them, by the ancients, had concluded them to be, you will excuse my sending you an observation or two, which occur'd to me relating to their natural history, and the considerable alterations, which they must have undergone, since they were first taken notice of in history, and what I think may have been the cause of these alterations.

These islands being so noted among the antients, I expected to find among the inhabitants a conscious esteem

esteem of their own antiquity, and of their appearance in history before the other parts of Britain were at all known. I was not without hopes of finding old towns and castles, perhaps inscriptions, and works of grandeur. But there is nothing of this kind; the inhabitants are all new-comers; not an old habitation worth notice; nor any remains of Phenician, Grecian, or Roman art, either in town, castle, port, temple, or sepulchre.

All the antiquities here to be seen are of the rudest Druid times; and, if borrow'd in any measure, from those eastern traders before-mention'd (superstition being very catching and infectious) were borrow'd from their most antient and simple rites.

We are not to think, however, but that Sylley was really inhabited, and as frequently resorted to antiently, as the old historians relate. All the islands (several of which are now without cattle, or inhabitant) by the mains of hedges, walls, foundations of many contiguous houses, and a great number of sepulchral barrows, shew, that they have been fully cultivated, and inhabited.

That they were inhabited by Britons, is past all doubt, not only from their neighbourhood to England, but from the Druid monuments; several rude stone pillars; Circles of stones erect; Kist-vaens without number; Rock-basins; Tolméns; all monuments common in Cornwall and Wales, and equal evidences of the antiquity, religion, and original of the old inhabitants. They have also British names for their little islands, tenements, and creeks.

How came these antient inhabitants then (it may be ask'd) to vanish, so as that the present have no  
pretensions



which at full sea there are ten and twelve feet of water. From the southern side of St. Martin's, (mark'd *A*) in the map annex'd, there stretches out a large shoal towards Trefcaw and St. Mary's; and from St. Mary's, a flat call'd Sandy-bar (mark'd *C*) shoots away to meet it; and between these two shoals there are but four feet of water, in the chanel call'd Crow-sound: All strong arguments, that these islands were once one continued tract of land, tho' now, as to their low-lands, over-run with the sea and sand. History confirms their former union. "The isles Cassiterides (says Strabo\*) are ten in number, close to one another; "one of them is desert and unpeopled, the rest are "inhabited." But see how the sea has multiplied these islands; there are now reckon'd one hundred and forty: Into so many fragments are they divided, and yet there are but six inhabited.

§ 3. But no circumstance can shew the great alterations, which have happen'd in the number and extent of these islands more than this; viz. that the isle of Sylley, from which the little cluster of these Cyclades takes its name, is no more at present than a high rock, of about a furlong over, whose cliffs hardly any thing but birds can mount, and whose barrenness could never suffer any thing but sea-birds to inhabit it. How then came all these islands to have their general name from such a small and useless plot? From the disposition of the rocks and islets here, and allowing the alterations before suggested, we may answer this question, which would otherwise be extremely difficult to solve.

From

From the southernmost hill of Brêhar I observed the Guêl hill, and the isle of Guêl (mark'd *I*) stretching away towards the little isle of Sylley, and with it making a curve, of which Sylley is the head-land: from the furthest hill of Brêhar, a promontory shoots out in the same sweep, and at the extremity, a vast rocky Tor call'd the Brêhar, behind which a ledge of many pointed rocks shew themselves above water, intimating the former connexion, and with what great devastations the furious ocean has dissolv'd it. So that Sylley, which is now a bare rock, and separated from the lands of Guêl and Brêhar by a narrow frith of sea, was formerly join'd to them by low necks of lands, being the rocky promontory of one large island now broke into seven.

To pursue this conjecture (if I may call that so, which has so many reasons to support it) a little farther: When all these seven islands, Sampson, Brêhar, Trefcaw, St. Elen's, Theon, St. Martin's, and St. Mary's, made but one, that one went by the name of Sylley, or some name of like sound and derivation\*; and, having some little islands scatter'd round it, imparted its name to them; whence, what were call'd by the Greeks Cassiterides, were nam'd by the Latin authors Sygdeles, Sillinæ, or Silures, from the British name, as I apprehend, which they found them call'd by

H 2

among

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\* Sûl-lêh signifies in Cornu-British, flat rocks, of, or dedicated to, the sun. So the mount of St. Michael was call'd in Cornu-British Din-sûl, the hill consecrated to the sun; and, as I conjecture, the vast flat rocks, common in these islands, had the like dedication: For, upon the surface of one of them, there are the remains of an antient Druid temple. It is one plane of rock, edg'd with nine vast stones (the rest taken away) planted in a circular line, measuring 172 feet from north to south, and 138 feet from east to west.

among the natives. I must go farther still, and observe that the promontory (at present call'd Sylley island) lying westernmost of all the high lands, was the first land of all the islands discern'd by traders from the Mediterranean, and Spanish coasts, and, as soon as discover'd, was said to be Sylley; nothing being more usual with sailors, upon their first seeing land, than to call the part by the name of the whole; of which I will not detain you with proofs. But, when this considerable island called Sylley was broke to pieces, the great portions became inhabited, and requiring distinct appellations, had first British names, as Brêhar, Trescaw, Enmor; but as soon as the regulars were placed here (probably in Athelstan's reign, or immediately after) were call'd, according to the religion of the times, after the names of particular saints. The chief division was intituled St. Mary's, in honour of the holy virgin-mother; the others dedicated to St. Nicholas, St. Martin, St. Theon, and so on; but this remarkable promontory (now Sylley isle) being in no wise fit for habitation or devotion, was dedicated to no saint, but left to enjoy its antient name; and, notwithstanding the modern Christian dedications, sailors went on in their old way; this high land is still call'd Sylley, and the islands in general are still denominated, from what was antiently their principal, Sylley isles.

It must have been a very dispiriting circumstance to the old inhabitants, to see the ocean so continually eating away their low-lands, in which they had their treasures of tin, their houses, and ports: but this gradual decay was not the only misfortune, which attended them,

them, neither will it account for the following phenomena.

§ 4. It has been mentioned before, that upon shifting of the sands in the chanel, walls and ruins are frequently seen: There are several phenomena of the same nature, and owing to the same cause, to be seen on these shores, as particularly a strait-lin'd ridge like a causeway, running cross the old town creek (mark'd *H*) in St. Mary's, which is now never seen above water; so that the subsidence, if any, has been different in different places. From the island of Sampson I saw the foundations of hedges (so we call the stone-fences of our fields, which are not built of masonry and cement) running on in a strait line cross the frith *E*, towards Trefcaw isle, till they were hid in sand; which sand, when in its full tide, has from ten to twelve feet water on it. Now we cannot suppose, that the foundation of these hedges was laid as low as high-water mark (for who would build fences upon so dangerous a level?) and if, at a medium, we suppose them to have been laid only six feet above the full tide, I am persuaded it will not be thought an unreasonable calculation. Here then we have the foundations, which were six feet above high-water mark, now ten feet under, which together make a difference as to the level of sixteen feet. To account for this, the slow advances and depredations of the sea will by no means suffice; we must either allow, that the lands inclosed by these hedges have sunk so much lower than they were before; or else we must allow, that, since these lands were inclosed, the whole ocean has been raised sixteen feet perpendicular; which

which last will appear, Sir, I believe, to the judicious, much the harder and less tenable supposition of the two.

Here then was a great subsidence; the land betwixt Sampson and Trescaw sunk at least sixteen feet, at a moderate computation. This subsidence must have been follow'd by a sudden inundation; and this inundation is likely not only to have destroy'd a great part of the inhabitants, but to have terrified others who survived, and had wherewithal to support themselves elsewhere, into a total desertion of their shatter'd islands. By this means, as I imagine, that considerable people, who were the aborigines, and carried on the tin-trade with the Phenicians, Greeks, and Romans, were reduced to the last gasp. The few poor remains of this desolation, by their necessary attention to food and raiment, must soon have lost sight of their antient prosperity; (for trade will produce plenty, and plenty rightly used will make people happy) and the faint remembrance, that was left of what the islands had been before, expired of itself, in an age or two, through the indigence of the inhabitants.

§ 5. That such an inundation has happen'd here, is still more plain, because these islands are no longer, what they were antiently, fertile in tin; nor are there any remains of such and so many antient workings as could maintain a trade, so greedily coveted by some of the antients, and so industriously concealed by others.

There are no mines to be seen in any of these islands, but only on one load (so we call our tin veins) in Trescaw island, and the workings here are  
very



very inconsiderable, and not antient; and indeed the sides of the cliffs, and the strata of bare rocks, do not shew, that ever there have been any considerable workings there for tin, or give us the least promising circumstance to encourage tin-adventurers. For among such numbers of rocks and cliffs, as I pass'd over in St. Mary's, and the off-islands, nothing surpriz'd me more, than that there should be so few veins in strata formerly so famous for tin. In the cliffs of Cornwall 'tis very different; you cannot walk on any beach, without perceiving veins of one sort or other, in the clay, rubble, or rock; but here in Sylley it is generally one continued rock, and the interstices so close, that scarce a knife can get between.

Besides the load above-mention'd in Trescaw, I saw a very narrow one in the same island under a place call'd Oliver's battery (marked K) but could perceive no metal, either by inspection, or by more than ordinary weight. I saw two veins, about two inches wide, running thro' the rocks on the back of St. Mary's pier; and a gentleman in company thought he had found one in Porthmellyn (L); and these are all we could discover, tho' our attention this way seldom left us.

There is one place on Dolphin-Downs, in Trescaw, where they work'd for tin in the antient manner, which was by laying open the ground in the same way as we now work stone quarries; and this is every thing I could perceive, in all these islands, which look'd like a working for tin. It must therefore be matter of wonder where the Phenicians, Greeks, and Romans, could have found such a plenty of that useful  
metal.

metal. Whatever resources they had from Cornwall, (formerly reckoned, as I have great reason to think, among the Cassiterides) great part of their tin must doubtless have come from these islands; but where it was found is uncertain. Nothing now appears above-ground, which can satisfy such an inquiry. The story of the Phenician vessel mention'd by Strabo to have purposely run ashore, and risk'd the men as well as lost the ship, rather than discover to the Romans the trade to these isles, is well known, and proves beyond all doubt the commerce to have been very advantageous. That the natives had mines, and work'd them, appears from Diodorus Siculus, *lib. 5. ch. 2.* and from Strabo (*Geog. lib. 3.*) who informs us, that Publius Crassus sailing thither, and observing how they work'd their mines, which were not very deep, and that the people lov'd peace, and at their leisure navigation, instructed them how to carry on this trade to better advantage: That is (if I understand him rightly) seeing their mines but shallow, yet well worth working deeper, taught them how to pursue the metal to a greater depth. The question then is, what is become of these mines? And how shall this question be answer'd, but by confessing, that the land, in which these mines were, is now sunk, and bury'd under the sea?

I am not fond of introducing earthquakes, or calling in the powerful subterraneous world to account for the superficial; but where there has been evidently a great subsidence of the earth's surface, I am very willing to refer to better judges than myself, whether it can be accounted for at all, without a previous trembling and concussion of the earth.

Proofs

Proofs are much better drawn from nature than tradition ; but yet I cannot help observing, that what nature declares in this case, tradition seems to confirm ; there being a strong persuasion in the western parts of Cornwall, that formerly there existed a large country betwixt the Land's-end and Sylley, now laid many fathoms under water. The particular arguments, by which they would support this tradition, I forbear to trouble you with, as they may be seen in Mr. Carew's *Survey of Cornwall*, p. 3. and in the last edition of Camden, p. 11. But, if I were to produce them, it should only be to prove, that there was such a tradition, and not as proofs of the matter of fact ; for of that I am very dubious ; the Cassiterides, by the most antient accounts we have of them, appearing always to have been islands. I know it is the opinion of some naturalists, that all islands were formerly join'd to the continent ; but we may, at least in my opinion, as well suppose the several parts of the continent, which are now separated by the branches of the sea, to have been once conjoin'd ; which will prove a great deal too much ; the present intermixtures of land and water being a much more gracious and social disposition, and for that reason more likely to be the true and original one, than so close and compact an union ; and therefore, where there are not shoals, and other evidences of a connexion (as there are none betwixt the Land's-end and Sylley) I think it the safest way not to suppose any.

But tho' there are no evidences, to be depended upon, of any antient connexion of the Land's end and Sylley, yet that the cause of that inundation, which destroy'd much of these islands, might reach also to the Cornish

nish shores, is extremely probable; there being several evidences of a like subsidence of the land in Mount's-bay. The principal anchoring-place, call'd a lake, is now a haven, or open harbour. The mount, from its Cornish name \*, we must conclude to have stood formerly in a wood, but now at full tide is half a mile in the sea, and not a tree near it; and in the sandy-beach betwixt the Mount and Penzance, when the sands have been dispersed by violent high tides, I have seen the trunks of several large trees in their natural position, the surface of their section worn smooth by the agitation of the water, sand, and gravel, as if cut with an axe, upon which, at every full tide, there must be twelve feet water; so that the shores in Sylley, and the neighbouring shores of Cornwall, are equal and concurrent evidences of such a subsidence, and the memory of the inundations, which were the necessary consequences of it, is preserv'd in tradition; tho' like other traditions, in proportion to their age, obscur'd by fable.

I should now make an apology, Sir, for troubling you with this sketch of the alterations, which the Sylley isles have suffer'd since the age of the antient historians: but because I have no other view in it than testifying my respect to that honourable Society, in which you deservedly hold so eminent a station, that must be my apology. I only beg leave to observe, that altho' these islands are neither of that extent they were formerly, nor fruitful of tin, nor stor'd

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\* Guavas lake, signifying the grey rock in a wood,

stor'd with such antiquities, as one might expect from their long commerce with the Phenicians, Greeks, and Romans; yet I found them well worth seeing: they are inhabited by an industrious, apprehensive people; their lands, fortifications, and commerce, are capable of great and easy improvements, and their harbours are of the utmost importance to the navigation of this kingdom. I am,

S I R,

Ludgvan, Dec. 21,  
1752.

Your most obedient servant,

Wm. Borlase.

*Extract of another Letter of Mr. Borlase to the  
Rev. Dr. Lyttelton, Dean of Exeter, and  
F. R. S.*

Read Feb. 8, 1753. **T**HAT there has been such a subsidence of the lands belonging to these islands, (as is before supposed) the present ruins of the islands testify. That this subsidence reach'd even to the Mount's-bay, and laid under water a great part of the low lands, then woody, there being now 10 feet water, (as at Sylley over the hedges, so here over the trunks and roots of trees) I have such convincing reasons to lay before you at a proper time, as will exclude all doubt. So that the shores in Sylley, and the shores in Cornwall, are equal proofs of such inundation, and the memory of it is preserv'd in tradition, tho', like other traditions, obscur'd in fable.

When this inundation happen'd, it is perhaps in vain to inquire; but two pieces of history occur to me, which possibly may lead us near the time. In the time of Strabo and Diodorus Siculus, their commerce seems to have been in full vigour; "Abundance of tin carried in carts," says Diodorus Siculus: — "But ten islands in all, (says Strabo) and nine of those inhabited." The destruction therefore of Sylley must be plac'd after the time of these authors; that is, after the Augustan age: but at what time after, I find nothing as yet can determine.

Plutarch indeed (of the *Cessation of Oracles*) hints, that the islands round Britain were generally unpeopled in his time. If he includes Sylley among them, then this desolation must have happened between the reign of Trajan, and that of Augustus. However that be, Sylley, tho' not intirely destitute of inhabitants, seems to have been, noted for two or three banishments during the empire of the Romans in Britain.

It continued neglected, till trade began to thrive, shipping increase, and naval wars to be carried on in this western world. Then its commodious situation at the opening into both the chanel's soon shew'd of what importance it was for Britain to possess it, and how dangerous it might be to the safety and trade of Britain, if in an enemy's hand.

This seems to be the reason, why Athelstan made a voyage to, and conquer'd, these islands. That prince was taught by his grandfather Alfred's wise maxims, that the proper and natural security of Britain lay in the royal navy, and its riches in traffick; and he saw, that neither of them could be well provided

vided for, if Sylley were not subdued as well as Cornwall.

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VIII. *An Account of Mr. Appleby's Process to make Sea-Water fresh; with some Experiments therewith; communicated to the Royal Society, by W. Watfon, F. R. S.*

Read Feb. 8, 1753. **T**HE making sea-water fresh has been frequently attempted, and several accounts thereof, from time to time, been communicated to the Royal Society. I thought it therefore not improper to lay before you Mr. Appleby's process for this purpose, in order to its being preserved in the journal-books of the Society. To this I have subjoined some experiments upon the water prepared in Mr. Appleby's manner, made by Mr. Michael Clark, operator at Apothecaries Hall, a person extremely well versed in the theory and practice of chemistry, at the desire, and under the inspection, of the censors of the college of physicians, to whom Mr. Appleby's process was referred by the lords commissioners of the admiralty.

Mr. Appleby's process.

**I**NTO twenty gallons of sea-water put six ounces of a fixed alcali, prepared with quick-lime as strong as *lapis infernalis*, and six ounces of bones calcined  
to

to whiteness, and finely powder'd. With a slow fire, draw off in a common still fifteen gallons.

Mr. Appleby conceives, that the alkali here employed is the best adapted to prevent the bituminous matter in sea-water from rising by the heat in distillation.

#### Mr. Clark's experiments.

**I**NTO a spoonful of the distilled sea-water he put twenty drops of a solution of silver in aq. fortis: He likewise did the same with the like quantity of common water distilled. There appeared no change in either, and both retained their transparency.

This demonstrates, that the distilled sea-water is by the process intirely freed from marine salt, or its acid spirit. For, if we take a spoonful of common distilled water, and add the least particle of sea-salt, with the point of a penknife, and then drop into the mixture one or two drops of the solution of silver, it will appear turbid and milky.

From the number of animal bodies constantly perishing in the sea, it may reasonably be suspected, that a volatile urinous spirit may be retained in this distilled water; and this is evident from the following experiment:

Into a spoonful of distilled sea-water drop ten drops of a strong solution of sugar of lead, and the mixture immediately becomes turbid and milky.

Into another spoonful of common distilled water, with two drops of spirit of sal ammoniac, add ten drops of a solution of sugar of lead: and this mixture had the same appearance with the foregoing.

If



If into a spoonful of common distilled water is dropp'd one drop of oil of tartar *per deliquium*, and then added ten drops of a strong solution of corrosive sublimate, the mixture will immediately become turbid and brown, and with a few drops of the solution of silver, it will be precipitated, and turn milky. It is a volatile alkali therefore, and not a fixed one, that is contained in this water.

The solution of silver will not discover a volatile alkali contained in water, but very plainly a fixed one.

A solution of sugar of lead will not discover a small quantity of marine salt or spirit, till we add more.

A solution of sublimate will manifest both a volatile and fixed alkali.

IX. *Extract of a Letter from Signor Camillo Paderni, to Dr. Mead, concerning the Antiquities dug up from the antient Herculaneum, dated from Naples, Nov. 18, 1752. Translated from the Italian.*

Read Feb. 8, 1753. **T**HE things, of which I have the charge, are many, and extraordinary; consisting of

Metals; that is, bronzes, silver and gold of all kinds, of excellent workmanship.

Beautiful cameo's and intaglio's.

Glass of all sorts.

Various

Various productions of the earth ; such as, grain, beans, figs, dates, nuts, pistachio's, almonds, rice, bread.

Colours for painting.

Medicines in pills, and other forms, with their marks.

A phial of oil.

Gold lace, perfectly well preserved, and extremely curious, on account of its being made with massy gold, spun out, without any silk, or other yarn.

Soap, bran, and a variety of other things, which it were tedious here to enumerate ; but there will be a relation of the whole published, which I shall immediately send to you ; as I hope you have received the book of Monseigneur Bajardi, already sent, altho' of little significance \*.

It is not a month ago, that there have been found many volumes of *papyrus*, but turn'd to a sort of charcoal, so brittle, that, being touched, it falls readily into ashes. Nevertheless, by his majesty's orders, I have made many trials to open them, but all to no purpose ; excepting some *words* §, which I have picked out intire, where there are divers *bits*, by which it appears in what manner the whole was written. The form of the characters, made with a very black tincture, that overcomes the darkness of the charcoal, I shall here, to oblige you, imitate in

\* The words in the original letter are, “ Come spero che avera ricevuto il libro de Monf. Bajardi in viatogli a benche non serva.”

§ I suspect an inaccuracy here in the original, and that he meant, “ excepting some bits, which I have pick'd out intire, “ where there are several words, &c.”

in two short lines; my fidelity to the king not permitting me to send you any more.

N. ALTERINS. DVLC  
DEM. CVRIS. CRUDE

This is the size and shape of the characters. In this bit there are eight lines. There are other bits with many other words; which are all preserved in order for their publication.

There have been found likewise very lately three beautiful statues of marble, and one of them excellent: Six heads of bronze, of which there is one, that gives hopes of finding the statue it belongs to. It is a young Hercules, of a kind of work, that has no fellow in the way of metal, having the hair finished in a surprising manner. Likewise several little figures of metal; a sistrum very neat and well preserved; and there is not a day passes, in which they do not bring to me some curiosities newly found.

X. *A Translation and Explanation of some Articles of the Book intituled, Theorie de la Figure de la Terre; by Monsr. Clairaut, of the Royal Academy of Sciences at Paris, and F. R. S.*

Read Feb. 15, 1753. **M**R. Short, in his account of Father Frisius's *Disquisitio mathematica in causam physicam figuræ et magnitudinis telluris nostræ*, having reported that philosopher's sentiments

on my reflections upon the same matter, without taking the trouble to examine whether they were founded upon the truth or not, I find myself under the necessity of laying before the Royal Society the passages of my book, which, having been misunderstood by F. Frisius, have occasioned the misconstruction, which he has made of my sentiments, either upon the trust I give to the actual operation made for discovering the figure of the earth, or Sir Isaac Newton's theoretical inquiries about the same subject.

The expressions of Father Frisius, referr'd to by Mr. Short, are as follow :

“ Quia tamen plerique omnes hucusque, aut nihil  
 “ pro figura telluris determinanda ex iis observationi-  
 “ bus deduci posse cum geometra celeberrimo Rug-  
 “ gero Boscovik autumârunt, aut exinde cum Ill.  
 “ Clairaut, Bouguer, aliisque, contra incomparabilem  
 “ virum ac prope divinum Isaacum Newton insur-  
 “ gentes, admirabilem ipsius theoriam factò minus  
 “ respondentem dixerunt, assignatamque in prop. 19.  
 “ lib. 3. *Princip. Mathem.* terrestrium axium pro-  
 “ portionem à vera absonam omnino esse, alios mihi  
 “ observationibus parum, alios nimis tribuere visum  
 “ est, omnes ferme oppositis erroribus peccâsse, ubi  
 “ res neque aurificis lance, neque molitoris, ut aiunt,  
 “ statera librandæ sunt.”

This, when compared with the propositions of my theory, which they are relative to, will appear, I hope, quite incoherent : and I cannot shew it more clearly, than by translating the last chapter of my book, to which F. Frisius refers the reader.

For

For the better understanding of that chapter, it is proper to know, that the chief results of the precedent inquiries are these theorems :

1. Supposing the earth in its former state composed of several fluids of different densities, and settled all in equilibrium by the laws of gravity and centrifugal forces, the surfaces separating the different mediums will always affect the form of a curve ; which is so near to the ellipsis, that it may be supposed so, without any error of the least moment.

2. That, in the case of the denser fluids being nearer to the center, as hydrostatics require, the spheroid will always be less flat than in the homogeneous one, and *vice versa*.

3. And as to the diminution of the gravity from the pole to the equator, it will always follow the opposite rule ; *viz.* if the spheroid be denser towards the center, the gravity will decrease in a less ratio than in the homogeneous spheroid, and *vice versa*.

4. That if  $\delta$  represent the fraction found out for the difference of diameters,  $\frac{1}{1+\delta}$  —  $\delta$  will express the total diminution of the gravity from the pole to the equator, not only in the case of the spheroid supposed originally fluid, but in any supposition of variation for the densities and proportion of the diameters of the beds, provided they be elliptical.

These premised, let us proceed to the said last chapter of the theory of the earth's figure ; in which the principles laid down in the preceding chapters are compared with the observations.

§ LXVIII. *For the diminution of the gravity from the North to the South.*

It has been seen in the preceding chapter, that when a spheroid is not supposed homogeneous, the diminution of the gravity from the pole to the equator will be greater than in the case of homogeneity. Hence, if my theory holds in our globe, the whole decreasing of the gravity will be equal to  $\frac{1}{230}$  or greater, and never less; since the ratio of 230 to 231 will (§ XXI.) express the ratio of the action of gravity at the equator and pole, when the spheroid is homogeneous.

And this conclusion of my theory quite agrees with experience; for, from all the observations relating to the gravity made in several places of the globe, either by actual measures of the second pendulum, or by the difference of duration of the same pendulum's vibrations, it appears, that the gravity decreases from the north to the south in a greater ratio, than it would be if the total diminution from the pole to the equator were only  $\frac{1}{231}$ .

§ 69. *For the proportion of the two diameters.*

Supposing, as in the precedent chapter, the earth originally fluid, it follows, from the § LXV. that the ratio of the two diameters cannot exceed that of 230 to 231; since, § XX. 230 to 231 is the ratio in the case of the homogeneous spheroid; and as the mensurations of the gravity cannot agree with the supposition of the homogeneity, the diameters of the earth ought to be in a ratio less than 230 to 231.

Without

Without adhering to the supposition of the earth's being formally fluid, but admitting, as in the chap. 3 and 4, all generality possible in the variation of density and ratio of diameters of the beds or strata laid down from the center to the surface, there will still happen a difference of the diameters less than  $\frac{1}{230}$ . For, by § L. the total diminution of the gravity from the pole to the equator being subtracted from  $\frac{1}{230}$ , the remainder is the difference between the diameters. Now the diminution of the gravity having been found greater than  $\frac{1}{230}$ , the ellipticity or difference of diameters ought to be less than that fraction, and consequently the ratio of diameters less than 230 to 231.

That consequence of my theory is not so happy as the preceding; for the degree measured in the north compared to that of France give the two diameters as 177 to 178, which ratio is greater than 230 to 231, instead of being less, as the theory would require.

As the measures made in the north have been performed with great care and exactness, their result seems at first to be preferred to that of my theory. But a reflection upon the errors unavoidable in actual measures, and an examination of the limits of these errors, will shew, that, without violating the measures, they would be brought nearer the theory, and even agree with it.

For, by a convenient calculation, it will be found, that a diminution less than 60 toises, made to the difference between the degrees of Paris and Tornea, would give the diameters in the ratio of 230 to 231. And if it be considered, what is the smallness of an error of 60 toises, when divided in two operations, which  
require

require so great a number of astronomical and geographical observations, it will be thought, that an error a little larger may be supposed, without disparaging either our operation, or Mr. Picard's; and thus theory and experience would agree.

Supposing, for example, that the difference between the degrees of Paris and Tornea has been found too great by 80 toises, the difference between the two diameters will come out of about  $\frac{1}{260}$ , which, subtracted from  $\frac{1}{115}$  gives  $\frac{1}{206}$  for the diminution of the gravity from the pole to the equator. And such a conclusion would agree pretty well with the observations made in France and Lapland with the excellent clock of Mr. Graham.

However, altho' the errors to be supposed in the operations, to reconcile them with my theory, be in themselves small enough, I shall abstain from asserting, that they have been committed. It is a fact not to be decided, till after the result of the observations, which are expected from Peru. For the great difference, which is to be found between the degrees of Quito and Tornea, is the only means of knowing, whether the diameters be less or greater than 230 to 231.

Were the question only to demonstrate the flatness of the earth, the measures of the degree of Paris and Tornea would be full sufficient; but to discover the true ratio of diameters, is what can be performed only by the comparison between the degrees, whose mutual distance is the greatest.

Such a ratio once fixed, if it happen to be less than 230 to 231, it will be very easy, by the preceding theory, to imagine some hypothesis for the inside of the



the earth, which shall agree with both theory and observation, whether admitting the supposition of the original fluidity of the globe, or not.

But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231, I own, that not only the theory established in this second part of my book must be abandoned, but it would be very difficult to reconcile the measures of the pendulums with those of the degrees in Sir Isaac's system. And I dare say, that the success in that case would hardly depend upon any natural hypothesis.

The subsequent LXX article containing only a proof, that the preceding theory agrees with any ratio between  $\frac{1}{23}$  and  $\frac{1}{9}$ , for the quantity, which expresses the excess of Jupiter's equator above its axis, there is no necessity for the translation of the arguments leading to a result so answering to the observations; and I pass to the conclusion of that article, which ends my book.

The preceding theory agreeing with all the measures of the pendulum, and observations of Jupiter's diameters, if, besides, it happen, that the measures expected from Peru give, when compared with those of Lapland, a difference of diameters less than  $\frac{1}{236}$ , this theory will have all possible confirmation, and the universal gravitation so well agreeing with the motions of the planets will also agree with their figures.

Now I beg every candid reader to examine, whether, in that chapter quoted by F. Frisius, I have too much relied upon the certainty of observations, and attempted to disparage Sir Isaac Newton's discoveries.

In the first place, I will ask of Father Frisius, if, before the operations, which I depended upon, were performed, I could establish any thing against their agreeing, or not, with Sir Isaac's proposition about the same matter?

He perhaps will answer, That my remark of the LXIX art. *But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231,* imports, that I was not thoroughly convinced, that what care soever would be taken by the gentlemen sent into Peru, they never would be able to measure their degree with a sufficient exactness, to conclude, from its length, compared with that of the other degrees, whether the diameters were in a greater or less ratio than 230 to 231; and consequently he will think, that my being in suspense about it was an offence against Sir Isaac's theoretical determination. Then, I request Father Frisius to tell me, why he is so good as to commend operations so void of use as those, which tended only to discover what was demonstrated before, and needed not to be confirmed, since it could not be invalidated.

Perhaps Father Frisius, in representing me as depending too much upon the observations, relied on these expressions of the LXIX art. *As the measures of the gravity cannot agree with the supposition of the homogeneity:* and I confess, that it seems to me impossible to reconcile the great number of all the measures of that sort with the table, which follows the homogeneity. For the simplicity of the means made use of in the performance of those measures cannot admit the errors, which should be supposed to bring them to Sir Isaac's theory: but as this theory is founded

on the homogeneity, which is only a mere supposition ; and as he has himself suspected, in his second and third edition, that the internal parts of the earth might be denser than those towards the superficies, I do not see how I oppose myself that illustrious philosopher, when I assume the same hypothesis, as he does. As I shall use all possible endeavour to understand F. Frisius's meaning, I hazard this conjecture. Seeing that I thought favourably enough of the exactness to be obtained in astronomy, when observations have been already made in great numbers, and with all possible care, to suppose them fit to let us know, whether the diameters are in a greater or a less ratio than 230 to 231 ; and being informed afterwards, that the operation made in Peru led those, who have made use of it, to imagine the spheroid flatter than the homogeneous, he concludes, that I cannot help thinking like them, and accordingly indulges himself in exposing, how much I over-rate the validity of observations, and how little I know the submission due to a proposition of Sir Isaac ; which, I must say, by the bye, that great man has never himself given as impossible to be opposed by experience. But yet I would ask of F. Frisius, wherefore he will guess at my sentiments, whilst I have not given room to know them on that point ? How can he know, whether, since the examination of all the measures, I have not found any way to reconcile them with the theory ? Which I say in no manner as a hint I intend to make any corrections in those measures, but merely to shew the little foundation, which F. Frisius had to represent me as he has done.

However difficult it may be to account for Father Frisius's expressions, I shall hazard yet another conjecture. His great zeal for Sir Isaac, for which he is certainly to be commended (if not blinded by that zeal) has hindered him from distinguishing between the different ways of opposing that great man's sentiments. Perceiving then, that my calculations (§ L. Part II.) had led me to a result quite different from Sir Isaac's assertion, (Prop. XX. lib. 3.) he was offended at my boldness to such a degree, that he was unable to examine impartially what I said; and, instead of discussing a mathematical question quite independent of any actual measure, wherein if I were mistaken, he would have forced every geometrician to condemn me, he has supposed, that I have built my argument upon an operation, which was not performed at the time when I wrote.

This conjecture would appear to me the true cause of F. Frisius's error, if it were not inconsistent with a proceeding of his towards Sir Isaac, which I will venture to relate. After F. Frisius has examined himself the 19 problem of the third book of the *Principia*, which is much less complicated than that I spoke of, the truth of which is incontestable, he finds, by his own mistake, a disagreement with the result of that proposition, and charges that illustrious author, without the least apology, with an error, which, says he, (quite from the purpose) is the sixth, that has been found in the same work, and also gives an enumeration of the five others, altho' they are not at all concerned in the question.

I cannot forbear saying, that the manner, in which I have proposed my remarks upon the 20th proposition

of Sir Isaac, has nothing of that flight way of treating so great a man; and as my utmost wish is to be judged on that account by the Royal Society, I shall relate what were my objections; which I cannot effect in a more concise and clear method, than by giving the translation of the article, which contains it.

§ LI. of the second part of the theory, &c.  
 “ *In which is seen what had induced Sir H. Newton.*  
 “ *to think, that the planets, when denser at the cen-*  
 “ *ter than at the surface, ought to be flatter than in*  
 “ *case of homogeneity.*

“ Some years ago I gave, in the *Philos. Transf.*  
 “ N° 449, the theorem of the precedent article;  
 “ and on this occasion I mentioned a passage of Sir  
 “ Isaac contrary to it. Not having at that time  
 “ looked into the second edition of his *Principia*, I  
 “ could not know what had engaged that illustrious  
 “ philosopher to think so; and far from suspecting  
 “ any mistake in his proposition, I was contented to  
 “ think, that the difference between our conclusions  
 “ arose from a different way of conceiving the inside  
 “ of the earth; and I imagined, that he had happen’d  
 “ to fall upon such a disposition of parts, as would  
 “ answer to his assertion. I then follow’d only his  
 “ commentators, and especially Dr. Gregory, shew-  
 “ ing, that his explanation of Sir Isaac’s conclusion  
 “ was wrong, as grounded upon a proposition, which  
 “ did not hold in the present case. For that pro-  
 “ position (which is, that the gravity at any point  
 “ of the earth is inversely as the distance from the  
 “ center) has only room, when the earth is homo-  
 “ geneous; and, consequently, ought not to be made

“ use of, when the density is greater towards the  
 “ center than at the superficies.

“ Since I have discover’d, that the theorem, the  
 “ demonstration of which I had given in the *Philos.*  
 “ *Transf.* for the case of beds supposed of the same  
 “ ellipticity, has room in an infinity of other suppo-  
 “ sitions, I have taken greater care to discover what  
 “ could have induced Sir Isaac to think, that the  
 “ earth is flatter, as the gravity is more decreasing  
 “ from the pole towards the equator; and I believe  
 “ I have found it out in the second edition of the  
 “ *Principia*, and it is, for having built upon the  
 “ same argument as Dr. Gregory.

“ In p. 386, after having observed, that the ex-  
 “ periments gave a diminution of two lines to the  
 “ second pendulum from the pole to the equator, he  
 “ argues thus: Since, says he, the case of homoge-  
 “ neity afforded only  $1 \frac{87}{1000} 4$ , the difference  $7 \frac{1}{2}$  miles  
 “ between the two diameters (which follow’d from  
 “ the same hypothesis) is to be magnified in the ratio  
 “ of  $1 \frac{87}{1000}$  to 2, and it will come out  $31 \frac{7}{12}$  miles  
 “ for the real difference. For, pursues he, the re-  
 “ tardation of the pendulum at the equator denotes  
 “ the diminution of the gravity in that place; and the  
 “ lighter the matter is there, the higher will it rise  
 “ to be equiponderant with that of the pole.

“ Further, p. 387, examining the measures of the  
 “ degrees of latitude made in France by M. Cassini,  
 “ by which the earth is higher at the pole than  
 “ at the equator by about 95 miles, he pretends,  
 “ that accordingly the pendulum should be longer at  
 “ the equator than at the pole by about half an inch.  
 “ And all that, methinks, shews the opinion, which  
 “ Sir

“ Sir Isaac was of, that, in any case whatsoever, the  
 “ equilibrium requires a gravity inversely propor-  
 “ tional to the length of the columns, which pro-  
 “ portion, as I hope to have evinced, is only demon-  
 “ strated in the case of homogeneity, and is not true  
 “ in general. Thus, what I argued in the *Phil.*  
 “ *Transf.* against Dr. Gregory, has also room against  
 “ Sir Isaac.”

By all that I have said, every body may judge, whether differing from Sir Isaac's sentiments on a point, which I had for so long a time examined, I did not express my disagreement with him in as decent a manner, as any one should, when speaking of so great a man. And in case the Royal Society thought some alterations were to be made in the form of my remarks, I declare, that I shall execute it, as may be prescribed to me by that illustrious company. But I cannot help thinking, that, unless those, who would examine my demonstrations, find some error in them, no alteration is requisite to be made in my expressions. I desire then either F. Frisius, or any geometrician, who thinks the question worth his examination, to take the trouble of reviewing my calculations, and to believe me ready to acknowledge my error, when shewn to me by a candid and impartial examiner.

*XI. An Account of a Storm of Thunder and Lightning, near Ludgvan in Cornwall, in a Letter from the Rev. Mr. Wm. Borlase, M. A. F. R. S. to the Rev. Dr. Lyttelton, Dean of Exeter.*

Revd Sir,

Ludgvan, Feb. 1, 1753.

Read Feb. 15, 1753. **S**INCE you have received only a short and general account of the lightning in my neighbourhood on the 20 of December last, I send you the notes, which I took on the spot; in which, you will excuse me, altho' I should be too minutely circumstantial, for fear I should omit any thing, which might contribute ever so little to give you, at this distance, a just idea of what happen'd.

About 8 that morning, I perceived the sky all of a sudden overcast in the east with very dark and red angry clouds; and during the continuance of the clouds, the wind very boisterous. These clouds pass'd away, and at intervals we had clear blue sky, and then large clouds again, attended with cold showers. About a quarter before twelve, there was neither rain nor wind, but sunshine. However, some flying thin clouds were at this time observ'd to join, and form a body in the north-west; and then ensued one of the most shocking peals of thunder I ever heard, attended here at Ludgvan with much lightning, but it did no harm. It was so also, in the high grounds of St. Hillary (next parish to this on the east) where a gentleman travelling found all the downs round him











him on a sudden full of lightning, but was rather frightened than hurt. But this thunder-cloud broke about three miles to the west of this place, in the side of Moelfra hill, in the parish of Maddern, where the marks of it (when I trac'd them) were as in the following sketches.

The first traces were an incision, or scratch, made in the turf, about three inches wide, and two deep, where the lightning coming up from the south-west at *A*, passing through the bank *B*, and issuing out of the bank in three streams, which united again at *C*, turn'd away to the north at *D*. About ten paces to the north of these breaches there are more marks of the same kind, but not in the same direction; for the lightning here came from the north-west, and, passing upwards, I observed the furrow, which it had made, to grow wider, and somewhat deeper, as it gain'd upon the hill, especially where it met with bank or stone; and some banks were five feet wide, which had their tops untouch'd, but were pierced thro' as with a bullet. This second furrow was (as all the rest) not in a strait line, but in a vermicular direction, and with its turnings led us to a Karn, or ledge of flat rocks, *E*. Here the lightning passing parallel to the side of the Karn, *H*, *L*, came in a direction from west by north, and meeting with a flat rock, *F*, *I*, (which in our country we call a quoit) standing on its edge, that southern part of which, *I*, directly opposed its passage, cut off all that end of this quoit at the dotted lines, split the quoit into two thinner ones, took off several splinters, and left large spots of sulphur (whitish like arsenic) on that side, whence the shivers were taken off. On the top of this quoit there is a remarkable  
incision,

incision, about three inches long, and as many wide, from which a piece of that quoit, of the same dimensions, was carried off, and in the same manner, as if a musket-ball had been fir'd at it. The lightning from this rock spread itself to the south, in two branches *G* and *M* breaking and rooting up some stones, and making its way clear under others, and appearing again on the other side. These last-mentioned furrows were ten inches wide, and a foot deep; besides which, we found several places in the hill, which had holes about a foot wide, and 6 or 8 inches deep, and several clods cut thin and clear off from ground: which shews, that as this lightning went like darts through banks and stones, and tore up the ground in many places like a ploughshare, so in other places it spread into an horizontal thin edge, which scooped up and carried off the little unevennesses of the turfy ground. The whole workings of this lightning were in length about a furlong from west to east. There were several people not far from the hill at this time, whose names I will not trouble you with. Two women, one half a mile, the other but a quarter of a mile distant, saw a smoke, at the Karn above-mentioned, as if several muskets had been discharg'd there. A third woman, not a furlong distant from the Karn, saw the town-place, where her house was, as it were all on fire; and during this dreadful thunder, the sheep on the hill ran to and fro, as if driven by a pack of dogs. This hill of Moelfra is the highest land between north and south sea in this part, about two miles from the former, and four from the latter. The wind was at west, and west-north-west.

This

The first thunder-clap was succeeded, in less than a quarter of an hour, by another, which broke at a village, in the parish of Gullval, called Trythal, about a mile and half to the south-west of Moelfra hill, and was attended with the following melancholy accidents; which, that you may the better apprehend, I have added a plan of the house, shewing the different stations of the persons, who suffered.

Thomas Olivey, a farmer of good substance and repute, was returned from the field, about a quarter before twelve o' clock, and had all his family round him in the kitchen, except his daughter, who was in the hall. There was a pan over the fire in the kitchen-chimney, full of boiling water. The farmer was sitting by the fire (at 1), and his wife on a bench before it (at 2); their only son, 23 years of age, was standing at the window (8), when it lighten'd much, and the first clap of thunder (spoken of before) follow'd. This clap of thunder was so violent here, that nothing was remember'd equal to it; and the back-door of the kitchen (6), which open'd to the north, quiver'd. The farmer called to his son, and desired him not to stand so near the window, lest the lightning should hurt his eyes; upon which the young man removed from the window, backwards, into the corner of the room, and sat down (at 3). For this, the apprentice-boy, laughing at him, was chid by his master, and luckily for him, sent out of the room, to take care of something without-doors. The lightning came from the west-north-west, and, falling upon the stack of the kitchen-chimney, which was about four feet square, and as much in height, of hew'd stone, carried it

M

clear

clear off from the house, and threw it into a pool of water twenty feet distant. In the chamber over the kitchen, directly beneath the top of the chimney, there was a little closet boarded in; all the boards were broken to pieces, the timbers of the roof shattered; also the bedstead in that chamber; of the chamber-partition two planks were forced, a large cloaths-press thrown, and the south windows of the chamber-floor (excepting one casement) all broken, and blown out. From the top of the chimney, and chamber-floor, it descended into the kitchen below, where the family was: the farmer saw no lightning, nor heard any thunder, after the first clap before-mention'd; but, as soon as he had given the orders to his apprentice-boy, as before-said, was struck senseless with the first flash, and thrown into the middle of the kitchen, and continued senseless for a quarter of an hour. As soon as he came to himself, he asked, who struck him? but had not the use of his arms; and felt an aching pain, shooting, as he described it, into his bones; and a brand-iron, which hung in the chimney, being thrown down into the pan of water, had dash'd the boiling water upon him to that degree, that his life was in extreme danger for more than a fortnight after.

Mrs. Olivey, sitting at (2), was struck down before the hearth (13). Both her shoes, tho' buckled on as usual, were struck off her feet; but her feet not hurt: and being neither burnt nor senseless, was able to cry out for help, but could not move; for she had no use of her under-limbs for a day and an half.

The



The farmer's brother was at the end of a long table (at 4) in the same room, and was only flung against the wall, about 3 feet distant, not hurt.

Mrs. Olivey's sister was near the back-door (at 6); a plank of this door was started, and beat in: she was struck senseless, and thrown twelve feet off against the settle (12), which stood against the south wall of the house.

The farmer's son was sitting (at 3); his coat and waistcoats (for he had two on) were torn into shreds, so that I could hardly distinguish where the pieces had formerly join'd; his shirt had a rent two feet long down the back, and was scorch'd; his left shoe torn from his foot; and the little toe of that foot so near cut off, that it hung but by a bit of skin; and he was quite dead. But, tho' reduced to this lamentable condition, as to his exterior, he was not mov'd from his seat, nor his face at all chang'd: his dog was lying at his feet, dead likewise, but never mov'd.

The farmer's daughter receiv'd the shock in the hall (at 7); was struck senseless, but revived soon; felt a trembling all over; her feet tickling, and partly benumb'd, and stiff, as if sleeping; but perceiving in the room a cloud of smoke, and hearing her mother cry, she made haste into the kitchen, which she found full of smoke, stinking like brimstone. The lightning had left a mark quite cross the clavel of the kitchen-chimney, about half an inch wide, in an undulating direction, broke thro' the partitions of the under-floor, thrown down the shelves, carried out all the south windows, forced up the stair-case, blown out the north window (10), miss'd or spar'd a clock, which stood close by the window

(at 11); and being somewhat spent, when it reach'd the hall (7), carried out the windows; moved not some Delft basons, which were in the south window, but forced the door of a beaufet (14), at the end of the hall, an inch and a half inwards; and shook the eastern wall of the house to the very foundation.

I propos'd only to lay before you the matter of fact, in the order of time, place, and degree, as it happen'd; but I cannot help admiring the different motions, shapes, and effects, of this lightning.

The clouds over Moelfra hill, and the village of Trythâl (a space of a mile and a half) were so heavily charg'd with lightning, that here they broke, both the first and the second time, and the thunder-claps were within a few minutes of one another, as being produced but by two portions of one and the same congeries.

The general tendency of this lightning was as the direction of the wind at that time; that is, from the north-west to the east, but where the principal explosions were (as at the hill, and the house) many branches spread off in all directions.

Nor were the shapes, in which it operated, less different than its motions. Sometimes, as it appeared to me at Ludgvan, it was point'd as a dart; in some places edg'd as a scythe, now but one thin sheet or stream, then two or three, and then one again. Now it fell as several separate balls of fire; but upon the house as a large gush, or torrent.

It was all fire, yet of different powers, according to the impregnation of its several portions. Subtil and penetrating as the electrical fire, it affected, shock'd, and permeated, all the human frame. Some  
parts

parts of it only scorched wood, but did not melt iron, as with lightning is very common: some tore the leather and cloaths; some cut and wounded, and some killed without wound or rent; and other parts of this lightning again, upon stone, wood, leather, cloaths, and flesh, only rush'd and forc'd with the power of air put into a violent agitation. All this happen'd in this place, and all in an instant: and altho' the cloaths were somewhat findg'd, as well as torn, and the young man's skin round his waist was also scorch'd, yet, from the general effects of this lightning in both places, I conclude, that it was rather swift, and irresistibly piercing, than inflammatory.

The house stands very high, without tree or hill near it. I went thither purposely to examine into the circumstances of this melancholy accident; and, after I had talk'd a little with the farmer and his family, and they had all (for my satisfaction) kindly enter'd into a detail of the particulars, the tears appeared in the eyes of some; others, even then, tho' almost a month after the misfortune, trembled; and all falter'd in their speech, and could not go on with their story, without frequent pauses. I remain,

S I R,

Ludgvan, Feb. 2;  
1753.

Your most obedient servant,

Wm. Borlase.

XII. *A second Letter of the Rev. William Henry, D. D. to the right honorable the Lord Cadogan, F. R. S. concerning the Copper Springs in the County of Wicklow in Ireland.*

My Lord,

Strabane, May 31, 1752.

Read Feb. 15,

1753

**I** RECEIVED this day the honour of your lordship's letter of the 18th, and must confess the great satisfaction, which I felt, in finding the account I sent of the copper mines in the county of Wicklow, acceptable to your lordship.

It would be too presuming in me, to think such a rough account of these curiosities worthy of the attention of so great and learned a body as the Royal Society: but, as your lordship is the most competent judge, and the truly great and learned are better pleased with important discoveries in nature, than with fine polishing, I leave it to your lordship to communicate the account I sent, to the Royal Society, in whatever manner you please. One thing I will pawn my reputation on, that every tittle in the account has the strictest truth to recommend it.

I did not see the iron shovel, which, by lying in the water, and being thereby incrustated with copper, gave the first occasion to this important discovery; for this happened some time ago; and the shovel is long since gone. But I was informed on the spot of this incident by the miners and workmen, of whom there were a great number. It was afterwards confirmed to me by the managers and proprietors of the mines.

As

As to the other point of the iron bars impregnating the copper, in the manner described in my letter, I examined it with the utmost attention, and was an eye-witness to it in all its progress, and so are thousands. I saw the masons laying a chain of new stone troughs, or pits, for the copper-water to run through. I saw men laying the iron bars on wooden rafters in these troughs. I had the iron bars lifted up out of some pits, where they had lain in the water from one to eight months, and saw them incrufted with the copper rust; and corroded to thin plates, in proportion to the time they had lain in the water. I saw some of the pits emptied, wherein the iron bars were wholly dissolved; and the labourers throwing up with shovels the copper, which lay on the flags in the bottom of the pit, like mud: and out of one of the heaps of copper-mud, which I saw thrown up, I took that very parcel of copper dust, which I sent to your lordship. It was like mud, as it lay wet in the heap; but became dust, as it dried. I also saw several pieces of copper, which I was informed were made out of this kind of copper-mud. And Dr. Dumainbray, in his philosophical lectures in Dublin, shew'd to us a plate of pure copper made in this manner; which led me to go to the mines, and there make this minute inquiry.

I must take notice of one improvement, which I omitted in my letter. To prevent any dirt or mud from being carried out of the mines, by the streams, which are let into the pits, where the iron bars are deposited, the stream, as it issues out of the level, is collected into a large deep basin, where all the dirt subsides;

sides; and the clear water only is from the surface of the basin let out into the pits.

If there be any thing, wherein I may further satisfy your lordship's inquiries in this or any other matter, your commands shall most chearfully be obey'd by

Your lordship's much obliged, and

most obedient humble servant,

William Henry.

XIII. *The Construction of the logarithmic Lines on the Gunter's Scale; by Mr. John Robertson, F. R. S.*

Read June 18, 1752. **H**AVING lately had occasion to treat on the construction of the Gunter's scale, I searched several books, wherein I suspected were contained the reasons of the common methods of laying down the logarithmic lines usually put on those scales: but not finding, either from my own search, or that of my friends, any satisfactory account of this matter, I drew up the following paper, to be laid before the Royal Society.

The Gunter's scale \* is an instrument almost universally known, and amply described by many writers; therefore

\* So called from its inventor Mr. Edmund Gunter, astronomy-professor in Gresham-College, from March 6, 1619, till his death, Dec. 10, 1626.

therefore I shall not take up your time in useless repetitions, but only shew, on what principles the divisions of the logarithmic fines, tangents, and versed fines, are usually protracted.

The line of numbers on these scales consists of two equal lengths, commonly called two radii; the first containing the logarithms of numbers from 10 to 100; and in the second are inserted those between 100 and 1000, or such of them, as can conveniently be introduced.

These divisions are taken from a scale of equal parts; such, that 100 make the length of one radius; and from this scale, the divisions for the fines, tangents, and versed fines, are also taken. Now, from this construction of the line of numbers, it is plain, that, as the numbers in one radius exceed those in the other, by one place in the scale of numeration; therefore the difference of their indices must also be unity: so that such numbers only, whose index differs by 1, can be estimated in a length of two radii: but, in a length of three radii, numbers, whose indices differ by 2, may be read; and a difference of 3 may be reckon'd in a length of 4 radii, &c. The tables of logarithmic fines, tangents, secants, and versed fines, are generally computed for a circle, whose radius is 10,000,000: therefore,

In the fines, the index 9 be-	8	'	"	0	'	"
longs to all between	90	0	0	and	5	44 36
The index 8	5	44	36	and	0	34 23
The index 7 to all between	0	34	23	and	0	3 27
6	0	3	27	and	0	0 21
	&c.					
	N					
	In					

In the tangents, the index 9 be-  
longs to all between 45° 0' and 54° 21'

And the indices, 8, 7, 6, &c.  
fall as in the fines.

In the verfed fines, the index	180	0	and	90	0
10 belongs to all between	90	0	and	25	51
9	25	51	and	8	7
8	8	7	and	2	34
7	2	4	and	0	45
6					

&c.

Now, as the length of the Gunter's scale admits of no more than two radii, or of such numbers only, whose index differs by unity; therefore, within this length, no more of the fines, tangents, or verfed fines, can be introduced, than those, whose index differs by unity: And as not only the greatest number among the fines and tangents, but also those more generally wanted, have the indices 9 and 8 differing by unity; therefore all the fines from 90° to 0° 34', and all the tangents from 45° to 0° 34', are those only, which are put on these scales; the divisions answering to the lesser fines and tangents being omitted for want of room. And this is the reason, why the sine of 90°, and the tangent of 45°, are limited by the same termination as the second radius on the line of numbers.

*To construct the line of logarithmic fines.*

From the scale of equal parts, take the numbers expressing the arithmetical complements of the log.  
fines



lines of the successive degrees, and parts of degrees, intended to be put on the scale, descending orderly from  $90^\circ$ : then these distances successively laid from the mark representing  $90^\circ$  at the right-hand end of the scale, will give the several divisions of a scale of logarithmic lines.

For, the ends of any scale being assigned, the progressive divisions of that scale are laid thereon from that end, which represents the beginning of the progression: or, the same divisions may be laid from the other end, by taking the complements of the terms to the whole length of the scale:

Consequently the arithmetical complements of the lines are to be laid from the division representing  $90^\circ$  degrees.

*To construct the line of logarithmic tangents.*

These are laid down in the same manner, and for the same reasons, that the lines were; the tangent of  $45^\circ$  standing against the line of  $90^\circ$ .

The divisions for the tangents above  $45^\circ$ , are reckoned on the same line from  $45^\circ$  towards the left-hand; or any tangent and its co-tangent are expressed by the same division.

Thus one mark serves for  $40^\circ$  and  $50^\circ$ ; and the division at  $30^\circ$  serves also for  $60^\circ$ ; that at  $20^\circ$  serves for  $70^\circ$ , &c. and the like is to be understood of the intermediate divisions.

For, as the tangent of an arc, is to radius;

So is radius, to the co-tangent of that arc.

Therefore the tangent is equal to the square of radius divided by the co-tangent.

And the co-tangent is equal to the square of radius divided by the tangent.

Now the radius being unity, its square is also unity.

Therefore the tangent and co-tangent of any arc are the reciprocals one of the other.

But the reciprocals of numbers are correlatives to the arithmetical complements of their logarithms.

Therefore the logarithms of a tangent and its co-tangent are arithmetical complements one of the other; and consequently will fall at equal distances from 45 degrees.

Therefore, in the line of logarithmic tangents, the divisions to degrees under 45 serve also for those above; both being equally distant from 45 degrees.

*To construct the line of logarithmic versed fines.*

As the greatest number of degrees will fall within the limits of the scale by beginning at  $180^{\circ}$ ; therefore the termination of this line is at  $180^{\circ}$ , which is put against  $90^{\circ}$  on the fines: and altho' the numbers annexed to the divisions increase in the order from right to left, yet they are only the supplements of the versed fines themselves.

Now subtract the logarithmic versed fines of such degrees and parts of degrees as are intended to be put on the scale, from the logarithm versed fine of  $180^{\circ}$ ; then the remainders taken from the foresaid scale of equal parts, and laid successively from the termination of this line, will give the several divisions sought.

The following table to every 10 degrees was constructed in the foregoing manner, and are the numbers  
to

to be taken from the scale of equal parts, for the degrees they stand against.

Degrees	Supplements of Verfed Sines	Degrees	Supplements of Verfed Sines	Degrees	Supplements of Verfed Sines
180	0,00000	120	0, 2, 94	60	0,00206
170	0,00331	110	0,173 7	50	0 74810
160	0,01330	100	0,23149	40	0,93190
150	0,03011	90	0,30103	30	1,17401
140	0,05403	80	0,38387	20	1,52066
130	0,08545	70	0,48282	10	2,21941

From this table it appears, that the least verfed fine, which can be introduced within the length of a double radius, falls between  $10^{\circ}$  and  $20^{\circ}$ , where the index changes from 1 to 2 ; which will happen about  $11^{\circ} 28'$ .

If a table of logarithm verfed fines to  $180^{\circ}$  are wanting, they are easily made by the following rule.

Take the logarithm fine of 30 degrees from twice the logarithm fine of ( $N$ ) any number of degrees; the remainder is the logarithm verfed fine of ( $2 N$ , or) twice those degrees."

For it is a well-known goniometrical property, that the fine of any arc ( $A$ ) is a mean proportional between radius ( $R$ ) and half the verfed fine of twice that arc.

Therefore, putting  $v$  for the verfed fine, and  $s$  for the fine;

The

The  $v 2 A = \left( \frac{2 ss A}{R} = ss A \times \frac{2}{R} = ss A \times \frac{2}{10} = \right)$

$ss A \times \frac{2}{7}$ ; radius being 10.

Or the  $\log. v 2 A = 2 \log. s A - \log. 5$ .

But when radius is 10, the sine of  $30^\circ$  is 5.

Therefore the  $\log. v 2 A = 2 \log. s A - \log. \text{ sine of } 30^\circ$ .

Most of the writers on this subject give the following rule for laying down the divisions of this line :

From the line of logarithmic fines, take the distance between  $90^\circ$  and any arc; that distance being twice repeated, from the termination of the line of versed fines, will give the division for twice the complement of that arc."

Thus the distance between  $90^\circ$  and  $20^\circ$  on the fines twice repeated, gives the versed fine of  $140^\circ$ ; or twice  $70^\circ$ , the complement of  $20^\circ$ . For the divisions, to be laid on this line, are the differences between the logarithm versed fine of  $180^\circ$ , and the logarithm versed fines of the successive arcs.

Now the difference between the logarithm versed fines of  $180^\circ$ , and of any arc  $2 A$ , is  $\log. \text{ ver. fine } 180 - 2 \log. \text{ fin. } A + \log. \text{ fin. of } 30^\circ$ .

Or,  $10,30103 + 9,69897 - \text{twice } \log. \text{ fin. of } A$ .

Or,  $20,00000 - \text{twice logarithm fine of } A$ .

Or the arithmetical complement of twice logarithm fine of  $A$ .

That is, the difference between the logarithm versed fine of  $180^\circ$ , and the logarithm versed fine of any arc, is equal to double the arithmetical complement of

of the logarithm sine of half that arc, rejecting the indices.

But, as these differences give the divisions to the supplements of the real verfed sines; therefore the arithmetical complement of the logarithm sine of any arc being doubled, will give the distance of the division for the supplement of twice that arc on the line of verfed sines.

Thus, for  $70^{\circ}$ , the logarithm sine is 9,97299

The arithmetical complement is 0,02701

Its double is 0,05402

Which is the number in the foregoing table standing against  $140^{\circ}$ , and is the supplement verfed sine of twice  $70$  degrees.

Now, as the arithmetical complement of the log. sines of arcs, are the distances on the line of sines between  $90^{\circ}$ , and the divisions to those arcs; therefore the distances between  $90^{\circ}$  and any arc, being twice repeated, will give the division of the supplemental verfed sine to twice the co-sine of that arc.

#### XIV. *A Letter from Mr. John Dollond to Mr. James Short, F. R. S. concerning an Improvement of refracting Telescopes.*

S I R,

Read March 1,  
1753.

**I**T is well known, that the perfection of refracting telescopes is very much limited by the aberration of the rays of light from the geometrical focus; which arises from two very different causes; that is, from different degrees

grees of refrangibility of light, and from the figure of the sphere, which is not of a proper curvature for collecting the rays in a single point. The object-glass is chiefly affected by the first of these; nor has there been yet any method discover'd for rectifying that aberration so, as in the least to remove the indistinctness of the image arising from it. We are therefore reduced to the necessity of contracting their apertures, which renders it impossible to magnify much without very long glasses.

But the case is widely different with regard to the eye-glasses; for, tho' they are very much affected by both the aberrations before-mention'd, yet, by a proper combination of several together, their errors may be in a great measure corrected. If any one, for instance, would have the visual angle of a telescope to contain 20 degrees, the extreme pencils of the field must be bent or refracted in an angle of 10 degrees; which, if it be performed by one eye-glass, will cause an aberration from the figure, in proportion to the cube of that angle: but if two glasses are so proportioned and situated, as that the refraction may be equally divided between them, they will each of them produce a refraction equal to half the required angle: and therefore the aberration being in proportion to the cube of half the angle taken twice over, will be but a fourth part of that, which is in proportion to the cube of the whole angle; because twice the cube of one is but  $\frac{1}{4}$  of the cube of two; so the aberration from the figure, where two eye-glasses are rightly proportion'd, is but a fourth of what must unavoidably be, where the whole is performed by a single eye-glass. By the same way of reasoning, when the refraction is divided

divided between three glasses, the aberration will be found to be but the ninth part of what would be produced from a single glass; because three times the cube of one is but one ninth of the cube of 3. Whence it appears, that, by increasing the number of eye-glasses, the indistinctness, which is observed near the borders of the field of a telescope, may be very much diminished, tho' not intirely taken away.

The method of correcting the errors arising from the different refrangibility of light is of a different consideration from the former; for, whereas the errors from the figure can only be diminished in a certain proportion to the number of glasses, in this they may be intirely corrected, by the addition of only one glass; as we find in the astronomical telescope, that two eye-glasses, rightly proportion'd, will cause the edges of objects to appear free from colours quite to the borders of the field. Also in the day-telescope, where no more than two eye-glasses are absolutely necessary for erecting the object, we find, by the addition of a third rightly situated, that the colours, which would otherwise confuse the image, are intirely removed: I say intirely removed; but this is to be understood with some limitation; for tho' the different colours, which the extreme pencils must necessarily be divided into by the edges of the eye-glasses, may in this manner be brought to the eye in a direction parallel to each other, so as, by the humours thereof, to be converg'd to a point in the retina; yet, if the glasses exceed a certain length, the colours may be spread too wide to be capable of being admitted thro' the pupil or aperture of the eye; which is the reason, that, in long telescopes, con-

○

structed

structed in the common manner, with three eye-glasses, the field is always very much contracted.

These considerations, Sir, first set me on contriving, how to enlarge the field by increasing the number of eye-glasses, without any hindrance to the distinctness or brightness of the image: And tho' others had been about the same work before, yet observing, that the five-glass telescopes, sold in the shops, would admit of farther improvement, I endeavour'd to construct one with the same number of glasses in a better manner; which so far answer'd my expectations, as to be allow'd by such persons, as are the best judges, to be a considerable improvement on the former.

Encouraged by this success, I resolv'd to try, if possibly I might gain some farther enlargement of the field by the addition of another glass: and by placing and proportioning the glasses in such a manner, as to correct the aberrations as much as possible, without any detriment to the distinctness, I have obtained as large a field, as is convenient or necessary, and that even in the longest telescopes, that can be made.

These telescopes with six glasses having been well received, and some of them being gone to foreign parts, it seems a proper time to settle the account of its origin; which is one of the motives, that has induced me to trouble you with this short sketch of the considerations, that gradually led me to its construction; and I am emboldened, Sir, to write thus much, from the many favours I have already received at your hands, as well as from a sense of your being a proper person to judge in such cases. And tho' I am sensible, that you are not unacquainted with the theory contain'd in this letter, yet foras-

much



much as the subject has never been fully treated by any author, I shall endeavour, as soon as may be, to draw up a more particular explanation of the aberrations of light by refraction; but shall add no more at present, only beg leave to take this opportunity of subscribing myself

Vine-Court, Feb. 21,  
1753.

Your much obliged and  
most humble servant,

John Dollond.

XVI. *A Comparison of different thermometrical Observations in Siberia ; by Mr. Wm. Watson, F. R. S.*

Read March 15,  
1753.

ACCORDING to the thermometrical observations made by Monf. Demidoff, at Soliskamsky, on the borders of Siberia, latit. 59, in the year 1751, the greatest degree of cold was upon Nov. 9, at 7 in the morning, when the thermometer, according to Fahrenheit's scale stood at 34 degrees below 0; which is 66 under the freezing point. This degree of cold, tho' much greater than what is ever observed in these parts, is little, when compared with the accounts given us by Professor Gmelin, in the introduction to the *Flora Sibirica*. This gentleman, who was professor of chemistry and natural history at Petersburg, was sent with several other learned men, to inquire into the natural history of Siberia, and was attended by some students, a painter or two, a miner, and other proper attendants. He continued nine whole years upon this expedition, and the observations he made, extraordinary as some of them are, with relation to their truth, are scarce to be doubted. The mercury in his thermometer, graduated according to De L'Isle's scale, often sunk in winter, in very southern parts of this country, as near Selinga, in lat. 48, to near 226, which is equal to 55 and a half below 0 in Fahrenheit's thermometer, and is 87 and a half below his freezing point. But the cold is often more intense than this, as appears by the experiments made at Kirenginshi,



			De l'ille	Fahrenheit	Reaumur	
			above o	below u	above o	below o
January	highest	20 day $0\frac{1}{2}$ p. m.	162	17,6		6,40
	lowest	31 day $7\frac{1}{2}$ mane	200 $\frac{1}{2}$		28,6	26,93
	at a medium		178 $\frac{1}{3}$		2,0	15,11
February	highest	28 day $0\frac{1}{2}$ p. m.	149	33,2		0,53
	lowest	13 day $7\frac{1}{2}$ mane	186		11,2	19,20
	at a medium		169 $\frac{1}{2}$	8,6		10,40
March	highest	29 day $0\frac{1}{2}$ p. m.	137	47,6		6,93
	lowest	1 day 11 p. m.	167	11,6		9,07
	at a medium		149 $\frac{1}{2}$	42,8		0,27
April	highest	30 day $0\frac{1}{2}$ p. m.	120	68,0		16,00
	lowest	9 day $5\frac{1}{2}$ mane	166	12,8		8,53
	at a medium		141	42,8		4,80
May	highest	8 day $0\frac{1}{2}$ p. m.	118	70,4	17,07	
	lowest	28 day $0\frac{1}{2}$ p. m.				
	at a medium	1 day $4\frac{1}{2}$ mane	157	23,6		3,73
June	highest	6 at $0\frac{1}{2}$ p. m.	105	86,0		24,00
	lowest	1 day 4 mane	143	40,4	3,73	
	at a medium	3 day 4 mane				
July	highest	31 day $0\frac{1}{2}$ p. m.	102	89,6		25,60
	lowest	29 day 4 mane	146	36,8		2,13
	at a medium		128 $\frac{1}{4}$	58,1		11,60
August	highest	23 day $0\frac{1}{2}$ p. m.	112	77,6		20,27
	lowest	28 day $4\frac{1}{2}$ mane	153	28,4		1,60
	at a medium		134 $\frac{1}{3}$	50,8		8,35
September	highest	1 day $0\frac{1}{2}$ p. m.	124	63,2		13,87
	lowest	18 day $5\frac{1}{2}$ mane	155	26,0		2,67
	at a medium		139	45,2		5,87
October	highest	10 day 11 mane	143	40,4	3,73	
	lowest	11 day $0\frac{1}{2}$ p. m.				
	at a medium	25 day $6\frac{1}{2}$ mane	169	9,2		10,13
November in the first 20 days	highest	26 day 11 p. m.	154	27,2		2,13
	lowest	1 day $0\frac{1}{2}$ p. m.	152	29,6		1,07
	at a medium	9 day 7 mane	205		34,0	29,33
December	highest	16 day $0\frac{1}{2}$ p. m.	179 $\frac{1}{2}$		3,4	15,73
	lowest	20 day 8 mane	150	32,0		26,13
	at a medium		199	10,1	26,8	0,72
			168 $\frac{1}{4}$			





Kirenginshi, where its sharpness was so great, that Professor Gmelin with difficulty staid at the door of his house, between three and four minutes.

Feb. 10, 1738, at 8 in the morning, the mercury stood at 240 degrees in De L'Isle; which is 72 below 0 in Fahrenheit. At the same place in 1736, Dec. 11, at 3 *p. m.* 254 in De L'Isle, almost 90 below 0 in Fahrenheit. Dec. 20, at 4 o' clock, *p. m.* 263 in De L'Isle =  $90 \frac{44}{100}$  below 0 in Fahrenheit.

Jan. 9, 1735, 12 at noon, 275 =  $113 \frac{65}{100}$ .

Jan. 6, 6 in the morning, 280 = 120 below 0 in Fahrenheit, and 152 below his freezing point.

Such an excess of cold could scarcely have been supposed to exist, had not these experiments demonstrated the reality of it; and Professor Gmelin assures us, they were made with all possible exactness, and agree with many others made in different parts of Sibiria by his direction.

It was not apprehended, that a greater degree of cold existed any-where, than that artificial one produced by Boerhaave, by means of ice and concentrated spirit of nitre, which sunk the mercury 40 degrees below 0 in Fahrenheit's thermometer; and this was supposed to be the point, beyond which no animal could bear it: and Mr. Gmelin's relation is the more extraordinary, as the French academicians under the polar circle mention the greatest degree of cold, observed by them, to be by Reaumur's thermometer 37 degrees, which nearly corresponds with 70 degrees below the 0 in Fahrenheit's.

XVII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the worshipful Company of Apothecaries for the Year 1752, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M. D. Societat. Pharmacæut. Lond. Soc. Hort. Chels. Præfekt. et Prælect. Botanic.*

Read March 15, 1501 **A** Cetosa Canopica minor. Lip.  
1753. Jussieu.

1502 Alcea Virginiana, Ricini folio. Fl. Lugd. Bat.  
2. 12.

Althæa Virginiana, Ricini folio. H. L. 23,  
Icon.

1503 Alysson incanum luteum, serpylli folio majus.  
Inst. R. H. 217. Thlaspi minus quibus-  
dam, aliis Alysson minus. J. B. 2. 928.  
Clypeola Lin. Gen. 650.

1504 Anguria Americana fructu echinato eduli.  
Inst. R. H. 107.

Anguriæ folio, fructu, Ovi figura, et magnitu-  
dine, ad maturitatem pallido, spinosis tuber-  
culis Momordicæ instar muricato. Pluk.  
Phyt. Tab. 170. f. 3.

1505 Astragalus orientalis, maximus, incanus, erec-  
tus, caule ab imo ad summum florido. Cor.  
Inst. R. H. 29.



[ III ]

- 1506 *Ballote*, folio *Geranii* *Batrachoides*. Gmel.  
Am. Ruth. 47.
- 1507 *Bidens Americana*, triphylla, *Angelicæ* folio,  
flore radiato. Plum.
- 1508 *Blitum fragiferum* majus vulgare; *Spinachia*  
*fragifera*. Ald. Hort. Farn. 85.
- 1509 *Cakile maritima*, ampliore folio. Cor. Inst.  
R. H. 49.  
*Cakile*, seu *Eruca marina latifolia*. J. B. 2.  
868.
- 1510 *Caryophyllus sylvestris* prolifer. C. B. P. 209.  
*Armeria prolifera*. Lob. Icon. 449.
- 1511 *Cassida peregrina*, *Melissæ* folio. Inst. R. H.  
182.
- 1512 *Caucalis major* *Daucoïdes* *Tingitana*. Mor.  
Umbel. 65.
- 1513 *Cistus femina*, folio *Salviæ*, supina, humi-  
sparsa. C. B. P. 465.
- 1514 *Cistus ladanifera* *Monspeliensium*. C. B. P.  
467.  
*Ladanifera*, five *Ledon Monspessulanum*, an-  
gusto folio, nigricans. J. B. 2. 10.
- 1515 *Clinopodium Alpinum*, roseum, *Satureiæ* folio.  
Bocc. Mus. 119.  
*Ziziphora*. Lin. Gen. 28.
- 1516 *Convolvulus*, *Linariæ* folio, affurgens. Inst.  
R. H. 83.  
*Volvulus terrestris* *Dalechampii*. Lugd. 1425.
- 1517 *Coris cœrulea maritima*. C. B. P. 280.  
*Coris Monspessulana cœrulea*. J. B. 3. 434.
- 1518 *Elichrysum orientale*. C. B. P. 264. *Stœchas*  
*citrina*, floris et magnitudine et colore spe-  
ciosa. J. B. 3. 154.

- 1519 *Eryngium planum minus*. C. B. P. 386.  
*Eryngium pusillum planum*. J. B. 3. 87.
- 1520 *Eupatorium*, caule erecto, foliis cordatis, serratis. Lin. Hort. Cliff. 396.  
*Eupatorium*, *Urticæ foliis*, *Canadense*, flore albo. H. L. 667.
- 1521 *Hypocoum*, tenuiore folio. Inst. R. H. 230.  
*Hypocoum*, filiquis pendulis teretibus æqualibus. Hort. Upsal. 31.
- 1522 *Jacea Tartarica*, laciniatis foliis. Gerber, Hort. Carolst. 104.
- 1523 *Jacea laciniata*, flore luteo magno, squamis calycum ciliaribus, splendentibus. Gerber tanais. 172.
- 1524 *Leucanthemum Alpin. Abfinthii folio*. Boerh. Ind. alter 107.
- 1525 *Leucoum*, vernal, perenne, album, majus. Inst. R. H. 221.  
*Hesperis Alpina*, five *muralis*, minor repens. J. B. 2. 880.
- 1526 *Lychnis sylvestris alba spica reflexa*. Bot. Monf. 171. Icon.
- 1527 *Lychnis viscosa*, foliis inferioribus Bellidi minori similibus, flore minimo, carneo aut rubro. H. R. Monsp.
- 1528 *Millefolium orientale*, foliis *Tanaceti incanis*, radiis pallide luteis. Boerh. Ind. alt. 112.  
*Achillea*, foliis pinnatis, foliolis lanceolatis, incis, serratis, subtus lanigeris. Lin. Hort. Cliff. 413.
- 1529 *Moldavica*, *Betonicæ folio*, floribus minimis pallide cœruleis. Amman. Ruth. 46.  
*Dracocephalum*, floribus verticillatis, bracteis oblongis,

oblongis, integerrimis, corollis vix calycem æquantibus. Hort. Upsal. 167.

1530 *Myosotis incana repens angustifolia*. N. D.

1531 *Nepeta, floribus obliquis*. Lin. *Lophanthus*.  
Amm.

1532 *Orobis sylvaticus, purpureus, vernus*. C. B. P.  
351.

*Galega nemorensis verna*. J. B. 2. 343.

153 *Phlomis, Hormini folio, floribus parvis suave-*  
*rulentibus villosissimis*. Amman. Ruth p. 39.

*Galeopsis maxima foliis Hormini*. Buxb. Cent.  
C. 1. p. 4.

1534 *Ptarmica orientalis, foliis Tanaceti incanis, flore*  
*aureo*. T. Cor.

1535 *Ptarmica incana humilis, foliis laciniatis, Ab-*  
*sinthii æmulis*. H. L. 510.

*Absinthium Alpinum umbelliferum*, Clus. Hist:  
340.

1536 *Refeda minor vulgaris*. Inst. R. H. 423.  
*Erucago Apula*. Col. 269.

1537 *Scandix Cretica minor*. C. B. P. 152.  
*Anisomarathrum*. Col. 1. 90.

1538 *Sclarea ficula, folio argenteo subrotundo*. Boerh.  
Ind. alt. 1. 163. *Æthiopis tota argentea,*  
*perennis lanuginosa*. Cupan. H. Cath.

1538 *Sclarea vulgaris lanuginosa, amplissimo folio*.  
Inst. R. H. 179. *Æthiopis*. Dod. Pempt.  
148.

1540 *Scorzonera, foliis laciniatis, caule erecto*. Inst.  
R. H. 477.

*Tragopogon, laciniatis foliis*. Col. Phyt. 2. 21.

1541 *Scrophularia Lusitanica frutescens, Verbenacæ*  
*foliis*. Inst. R. H. 167.

*Scrophularia*, foliis lanceolatis, obtusis, ferrato-  
dentatis, pedunculis bifidis. Royen. Fl.  
Leyd. Prod. 294.

1542 *Stœchas folio ferrato*. C. B. P. 216. *Laven-  
dula foliis crenatis*. T. 198.

1543 *Symphytum, consolida major, flore purpureo*.  
C. B. P. 259.

*Symphytum magnum*. J. B. 3. 593.

1544 *Thlaspi montanum sempervirens*. C. B. P.  
106. *Iberis foliis linearibus acutis integer-  
rimis*. L. H. Clif. 330.

1545 *Tragapogon hirsutum*. C. B. P. 274.  
*Tragapogon Apulum, humile, hirsutum, lu-  
teum*. Col. p. 1. 233.

1546 *Valeriana maxima Pyrenaica, Cacaliæ folio*.  
Fagon. Tourn. 131.

1547 *Verbascum humile Alpinum, villosum, Borra-  
ginis flore et folio*. Tourn. 147.  
*Sanicula Alpina, folio Borraginis, villosa*. C.  
B. P. 243.

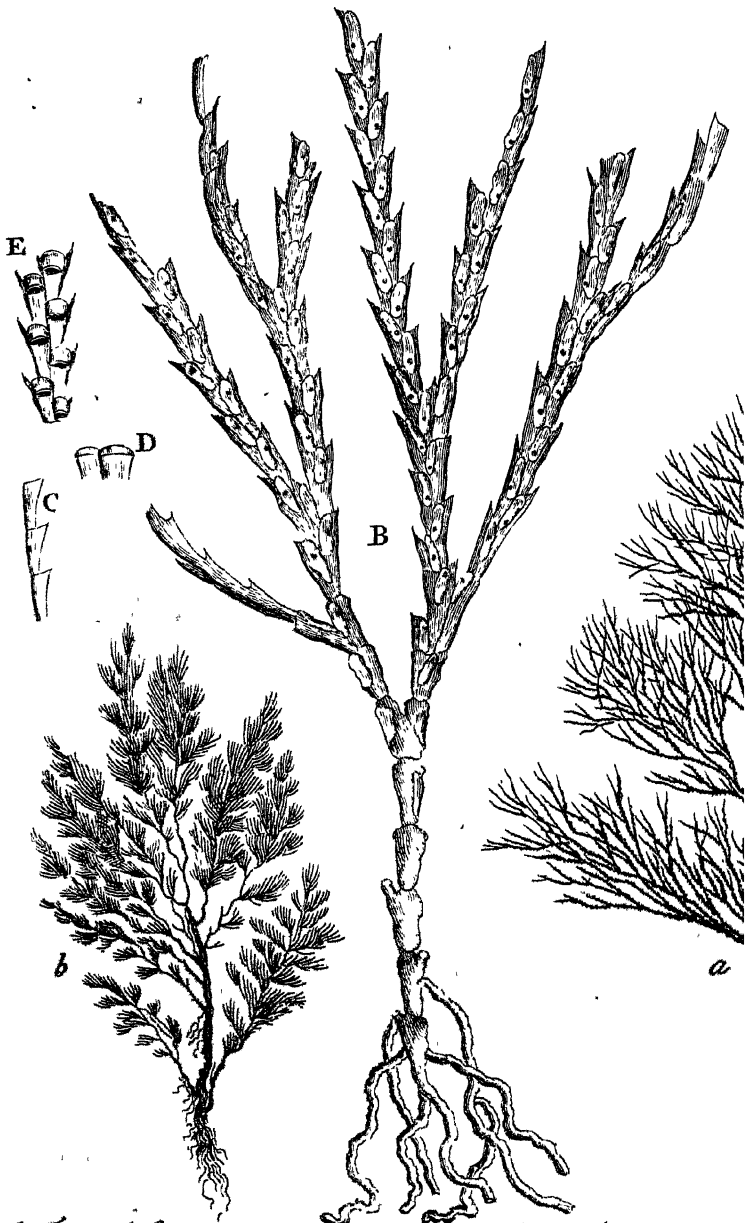
1548 *Veronica Auftriaca, foliis tenuissime laciniatis*.  
T. 144.  
*Chamædrys spuria tenuissime laciniata*. J. B.  
3. 387.

1549 *Veronica orientalis, minima, foliis laciniatis*.  
T. Cor. 7.

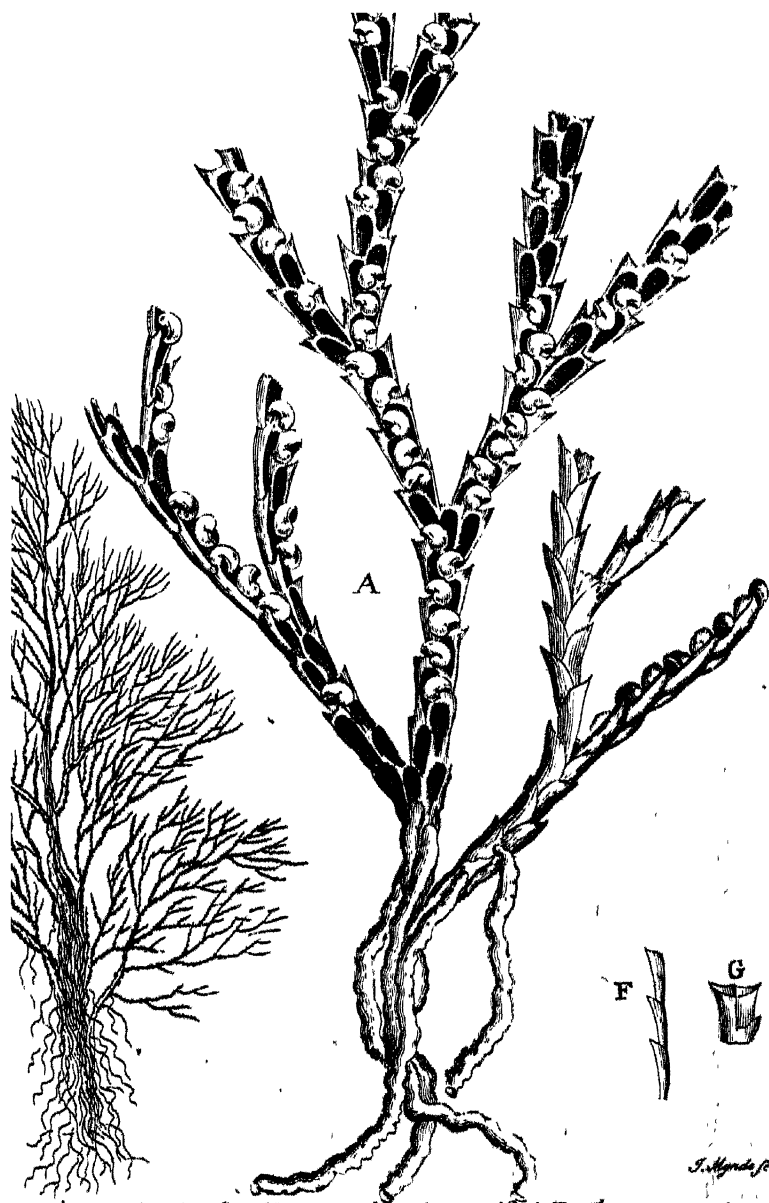
1550 *Waltheria, foliis cordato-ovatis, ferratis*. L.  
H. Clif. 342.

*Donospermalthæa arborescens villosa, flore ma-  
jore*. D. Isnard. Act. Reg. Scient. 1721.  
p. 278, Tab. 14.





*b. Theophrastus feathered Coralline B. a branch magnified with its tubuli.*



*a.* The Snail bearing Coralline. *A.* a branch magnified. *F.* The Eggs turned to  
*F.* the upright section. *G.* the cross section. (*testaceous animals*)





XVIII. *Observations on a remarkable Coralline, in a Letter from Mr. John Ellis to the Rev. Thomas Birch, D. D. Secret. R. S.*

S I R,

Read March 17, 1753. I BEG leave to present you with some observations, which I have made on a coralline, that I lately received from my curious and worthy friend Mr. Peter Collinson. It appears, from its size and firmness, to belong to a warmer climate than this, and is probably American.

We find some of the same genus, but of a different species, of this coralline, on our own coasts; but they are smaller, tenderer, and more transparent. There is one particularly, which comes very near this, called by Dr. Dillenius, in the third edition of *Ray's Synopsis*, p. 37, N. 20, Tab. II. Fig. 1. *Corallina pumila erecta ramosior*: and in *Buddle's Hortus siccus*, in the late Sir Hans Sloane's collection, there is a specimen like it, but not so fully advanced in its ramifications: this he calls *Fucus minimus hirsutus fibrillis herbaceis similis*, from *Doody's Appendix to Ray's Synopsis*, p. 330.

This curious sea production, which has the appearance of a plant, arises first from many small vermicular wrinkled tubes, by which it appears to have adhered, like ours, to rocks, shells, fucus's, or other submarine substances. These tubes uniting form a sort of stems, which, as they rise, insensibly change into rows of cells: these stretch out into many regular

dichotomous branches; each branch is made up of two rows of cells united together, and these cells placed in such a manner side by side, that each cell joins two others on one side, and the bottom of one is inserted in the top of the other. Their openings or faces look one way: they are nearly of an egg-shape, a little compress'd before: the broadest part is uppermost, and bends a little forward: the top of each is fortified by two angular points or spines.

By attentively viewing many specimens of this genus of corallines in the microscope, that have been taken out of the sea at different seasons of the year, I have observed the progress of nature to be pretty nearly thus:

The tubuli, or first beginning of the corallines in the younger state, are found full of a yellow soft substance, which soon decays: in the more perfect state they are clear and transparent.

The cells, which communicate with these tubes, have in the spring black specks in each, which I take to be the embryo of the future production. During this very tender and minute state, the opening of each cell is cover'd with an extreme fine transparent membrane, the use of which no doubt is to cherish and protect it.

These specks in time swelling into spherical testaceous bodies (as they are often found in summer) burst through this membrane, and sit in the front of the cell, supported by an umbilical ligament, which is fasten'd to the bottom of the inside of each cell or matrix, till they come to maturity, which seems to be the case in the microscopical drawing I have sent you: wherein you will observe, they appear to be rows of very small sea snails, or rather testaceous bodies,  
of

of the shape of a nautilus, ready to drop off, and provide for themselves. In the same plate you have a microscopical drawing of one of the English corallines of the same genus, with the embryo specks in each cell.

I must further add, that I believe, if the curious, with good microscopes, at the sea-side, and at different seasons of the year, would strictly examine many of these beautiful sea-productions, hitherto claim'd by the botanists, they would find, that several of the testaceous tribe proceed from some kinds of the larger corals, as well as, I am persuaded, they will find, that many owe their original to the smaller corallines: and we are the more encouraged to try, since we observe, that various shapes and stages of the same animal are no new thing in the laws of nature.

S I R,

Your most obedient humble servant,

John Ellis.

XIX. *An Account of some uncommon fossil Bodies, by Mr. Henry Baker, F. R. S.*

Read March 29, 1753. **T**HE fossil bodies I have now the honour to lay before this Royal Society, I have never met with before, nor remember any description of. They were sent to me from Oxford, by Mr. William Frankcombe, a young gentleman residing there, who is very diligent in searching

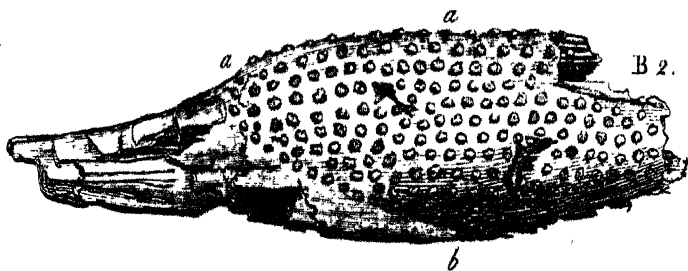
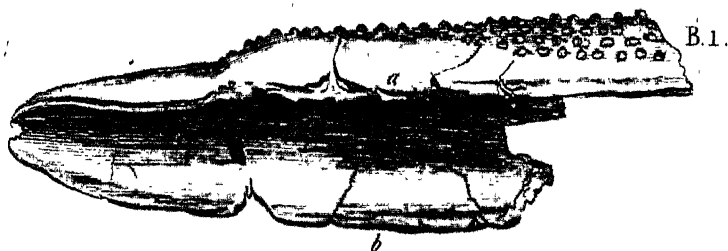
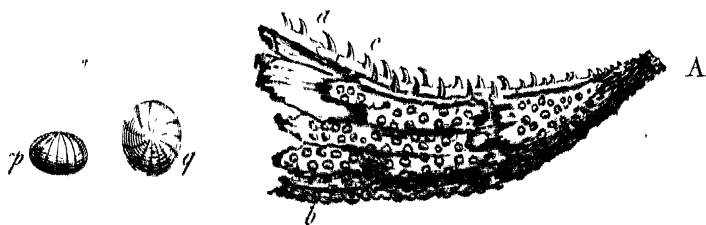
searching after curiosities of this nature. He found them himself, but could not get them out of the bed they lay in without breaking them in many pieces: though he has glewed those pieces so well together, that one may judge of them nearly as well as if they had not been broken.

As I must return them to Mr. Frankcombe when they have been examined by you, I have caused Drawings of them to be made, for the satisfaction of those who may never have an opportunity of seeing them; to which Drawings I shall refer in the description of them I am going to give. I shall then inform you, from his letter that accompanied them to me, where and amongst what other kinds of fossil bodies they were found, with other particulars relating to them. And afterwards I shall venture to lay before you a few conjectures concerning them.

*Plate VI.* shews these curious fossil bodies at more than half their real bigness. They are only three in number, though there are four figures, one of them being drawn in two positions. They are evidently of a boney substance, made black, most likely, and rendered brittle, by some mineral steams or juices, though not corroded by them. Two of these bodies (*A* and *B*) have the greatest part of their outer surface studded, as it were, with pretty regular rows of tubercles, about the size of the heads of small nails, rising to a blunt roundish point, nearly one twelfth of an inch above the surface they issue from. Many of them appear radiated very prettily from the base to the apex; and perhaps they have all been so, tho' in some the lines are not now seen, and may have  
been







*D. E. Baker delineavit.*

*J. Mynde Sc.*





been obliterated by time. These tubercles are of a fine shining glossy black colour, and of a much closer and harder substance than the bone from which they rise.

Figure *A* represents one of these fossil bodies, whose length from end to end is seven inches and a quarter ; on the sides from *a* to *b* its breadth is two inches. The width of that part where the teeth are placed at *c* about seven eighths of an inch ; but it gradually decreases, as does likewise the breadth of the sides, towards the smaller end, which was probably about an inch longer than it now appears, and terminated in a point. The tubercles are largest in the broadest part, and the farther they are from the teeth, near which they are small and flat: they likewise lessen towards the smaller end, which is ridged for about an inch, and without any tubercles.

The under-part of this body is placed uppermost, for the sake of shewing its teeth to the best advantage. There are two rows, running longitudinally, on a little rising in the middle, with no great regularity, and ending in one row of very small ones. The largest are about a quarter of an inch in length, hooked, of a shining black colour, having still the natural polish, and being extremely sharp and perfect. The sides of this fossil have swelled out, and been naturally more rounded than they are at present: for they plainly appear to have been crushed and compressed together by some foreign force.

*B* 1, is a fossil body, ten inches in length, one part of which is rounded, and the opposite part hollowed: this figure shews the hollow part, which from *a* to *b* is more than one inch and half over ; the  
chane

channel runs its whole length, and where deepest is an inch and half over, but it gradually grows shallower and narrower towards the smaller end. The sides are a quarter of an inch in thickness.

*B 2*, shews the same fossil body with the rounded part upwards. Its sides from *a* to *b* are two inches. Great numbers of black shining tubercles, of the kind described Fig. *A*, but in general larger, and with less variation in their size as to one another, are disposed in rows, pretty regularly in the manner shewn in the picture. Many of them appear starry or radiated with several fine lines from the base to the apex, which lines rise a little, and in some positions to the light appear of a whitish colour.

Two separate figures of these tubercles are given (*p, q*) to make this account the better understood. One is a side and the other a front view. They are shewn magnified about eight times.

*C*, is a fossil body, much more solid and weighty than the former two. Its length is ten inches. It is rounded on the upper part, where the sides in the broadest place are one inch three quarters: the under part has a hollow or channel one inch and an eighth in depth, seven inches and a half long, one inch and a half over, its bottom rounded. From *a* three inches and a half to *b* is quite solid, and at *a* in width one inch and a quarter, whence it goes tapering to *b*, where it is broken off so blunt as to shew, that it must probably have extended four or five inches farther. In this solid part *c* stand many small teeth in rows, but not quite regular; some rows having but two, some three, and others four. They begin an inch distant from the channel, and went probably to the extremity that is broken

broken off. They are black and shining like those in *A*, but the points somewhat broken; tho' when whole they must have been less hooked, and much smaller than they.

The rounded part of this fossil body has no tubercles like the other two, tho' it is plainly a species of the same kind with them, but is pretty strongly furrowed, and the ridges have the same black glossy polish as their tubercles.

Mr. Frankcombe writes, " That he met with these  
 " two bodies *A* and *B* in a pit, on the right-hand  
 " side of the road, as you ascend Shotover-hill from  
 " Oxford. The uppermost stratum in this pit consists  
 " of a yellow sandy earth; the next a brownish  
 " clay; then a regular stratum of large stony nodules,  
 " about twelve inches thick; then a dark blue clay,  
 " of about ten feet; and immediately under a rock of  
 " free-stone. About two feet above the free-stone  
 " were found the fossils *A*, *B*. The first was found  
 " at twice; the second in searching to complete the  
 " first, and both of them in many small pieces,  
 " as is evident from the bodies themselves, which  
 " he carefully joined with some thick gum-water.  
 " That the first is of its proper shape and figure  
 " plainly appears from the regularity of its tubercles:  
 " and the second is as he saw it himself in the stratum.  
 " In this clay are found bones of several  
 " kinds, oyster-shells, *Ammonitæ*, crustaceous shells,  
 " *Selenitæ*, and *Belemnitæ*.

" The cliffs on the right-hand side of Pyrton-  
 " Passage over the Severn, Gloucestershire, afforded  
 " the body marked *C*. This, says he, which was  
 " likewise found in a stratum of blue clay, not unlike

“ that at Shotover, and also in several pieces, appeared  
 “ different from the others in nothing, but in the want  
 “ of tubercles, and I flatter myself will serve to throw  
 “ no small light on the subject. His being not quite  
 “ so conversant with these cliffs as with the pit at  
 “ Shotover, prevents his speaking of them so parti-  
 “ cularly as he could wish.”

The general appearance of these fossil bodies gives reason to conjecture, that they are bones belonging to the head or snout of some animal of the fish-kind, or perhaps of some sort of lizard, alligator, or crocodile.

The piece *A*, whose sides are a little crushed, was found in Oxfordshire, together with the piece *B* 1 and *B* 2, and may probably have been part of the same head: and if so, it should seem from the two rows of teeth along its middle to have been the upper part of the head or snout: for some kinds of fishes have teeth in the palate or upper part of the mouth, but we know of none that have teeth along the middle of the lower part: there a tongue most commonly is placed, and the piece *B* has an hollow or chanel well adapted to contain a tongue. The teeth in the palate of the *lupus piscis*, and likewise of some other fishes, are frequently found fossil, of various sizes and shapes, being what are called (very improperly) *Bu-fonitæ*. When the two pieces *A* and *B* are brought together, their size, figure, and appearance, greatly strengthen the above conjecture: and 'tis worth observing, that the teeth are hooked inward, to prevent the prey when taken from escaping.

The piece *C*, found in Gloucestershire, serves likewise to confirm the same opinion: for the tothing in the middle thereof almost proves that part to have  
 been

been the palate of some animal ; an animal of the same genus too with *A* and *B* ; tho' its having no tubercles, and being more solid, shew it to have been of some different species.

I remember not any fossil bodies like these mentioned by authors, nor can I point out any animal, to which they may with certainty be imputed. Animal substances, before unknown, are met with frequently in the bowels of the earth : for the inhabitants of seas and rivers have been hitherto so imperfectly described, that we know but little of their internal structure ; and many sorts we have never seen or heard of. — Amongst the great numbers of fossil shells we find, how few of their correspondent kinds are seen in the best collections of recent ones ? and what a variety of marine bodies, as well as parts of land-animals, are frequently dug up in this island, of kinds never found recent in our seas, or inhabitants of our clime, such as *nautili*, *ammonitæ*, &c. the teeth and bones of elephants, the palmed horns of the elk, of the morse-deer, and many others ?

I shall be glad to have this subject considered by somebody more able to give you information ; tho' no one wishes more the prosperity of this Royal Society, or can be with greater zeal,

Gentlemen,

Strand, March 29,  
1753.

Your most obedient servant,

than

Henry Baker.  
XX.

XX. *An abstract of a Discourse intitl'd, The history of the emperor Tetricus, explained and illustrated by medals; written in French by Mr. Claude Gros de Boze, keeper of the medals in the French King's cabinet, etc. and sent by him to Dr. Mead, to be communicated to this Society. By John Ward, LL.D. Rhet. Prof. Gresh. and V.P. R. S.*

Read April 5,  
1753.

AS the emperor *Tetricus* governed some years in *Gaul*, his reign does in a manner make part of the history of that country. But the accounts given of him by antient writers being very confused and imperfect, this learned and skilful antiquary has attempted to rectify and clear them up from medals. And as to those of *Tetricus* himself, the gold ones, as he observes, are in general exceeding scarce; and no medallion of this emperor in any metal was known, till very lately, when he procured one in gold for the French King's cabinet, a draught of which transmitted by him accompanies this paper (1). But tho Mr. *de Boze* professes only to give the *History of the emperor Tetricus*; yet such was the unsettled state of the Roman affairs at that time, as made it necessary for him to introduce it, by reciting a variety of incidents relating to other persons, which prepared the way for his advancement to that dignity.

He

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(1) See TAB. II. Fig. 2.

He therefore begins his narrative with observing, that the *Gauls*, after they had been conquered by *Julius Caesar*, continued pretty quiet under the Roman government, till the time of *Gallienus*; whose reign was so weak and scandalous, as to render him the common object both of hatred and contempt. *Valerian*, his father, had been created *Augustus* some years before his expedition against the *Perfians*; by whom he was defeated, made a prisoner, and treated with the utmost indignity (1). This event, which happened in the year 260 of the vulgar aera (2), astonished the whole empire. And several nations, who were not concerned on either side, then offered their assistance to the *Romans*. Likewise divers princes in alliance with *Sapor* or *Sapores*, the Persian king, advised him to make a good use of his success, and secure to himself a solid and lasting peace, by releasing his illustrious captive; but the haughty monarch would not hearken to any of those salutary counsels.

*Gallienus* had the title of *Augustus* before that unhappy expedition, and being now at liberty to indulge his excesses without restraint, he shewed no concern for the misfortune of his father. Insomuch, that upon the first news of it, affecting the air of a philosopher, he said, *Sciebam patrem meum esse mortalem*. And while a general greif spread itself among persons of all ranks, he alone, as the historian expresses it, *Specie decoris, quod pater ejus virtutis studio deceptus videretur*,

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(1) *Trebell. Pollio, in Valeriano. Eutropius, Lib. ix. c. 6. Aurel. Victor, in Epitome, cap. 32.*

(2) *V. Rom. 1013.*

*videretur, supra modum laetatus est* (1). But altho this pretended philosopher used no indeavours to procure his father's release, he could not, as Mr. *de Boze* thinks, free himself from some remorses of conscience on that account; and therefore occasioned a false report to be given out concerning his death, and divine honors to be conferred on him, that he might be no longer talked of (2). The *Persians* in the mean while, attentive to what passed at *Rome*, indeavoured to render *Valerian* daily a miserable spectacle; stript of his imperial ornaments, loaded with chains, and exposed to that contempt, that *Sapor* made a footstool of his neck, whenever he mounted his horse (3); in which deplorable state he continued the remainder of his life.

As disdain and resentment often succeed murmurs, the barbarous nations easily threw off the yoke, which fear had imposed on them; the most submissive provinces thought it a disgrace to obey *Gallienus* any longer; and the armies in most of the Roman territories chose themselves emperors, many of whom thro emulation soon destroyed one another. *Trebellius Pollio* has given a short history of them in a continued series; whom for the sake of a round number he calls the *Thirty tyrants*, as not having been acknowledged by the senate.

The *Gauls* were in hopes, either not to have embarked in this general conspiracy, or however to have  
been

(1) *Trebell. Pollio*, Gallieni duo.

(2) *Ibidem*.

(3) *Laëtant*. De morte persecut. cap. 1. *Aurel. Victor*, Epitome, c. 32. *P. Orosius*, LIB. VII. c. 22.



been the last in it. *Postumus* (1) had governed them almost three years by the appointment of *Valerian*; who writing to them in his favour, among other encomiums there given him, uses this expression, *Virum, quem ego prae caeteris stupeo* (2). He had before been governor to his son *Gallienus*, and preferred to *Aurelian*, who was afterwards emperor, on account of his mild and gentle disposition; as *Valerian* himself intimates in a letter to *Antoninus Gallus* the consul, where speaking of *Aurelian* he says, that he was fearful, *Ne quid etiam erga filium severius, si quid etiam ille fecisset (ut est natura pronus ad ludicra) saevius, cogitaret* (3).

*Gallienus* was in *Gaul*, when he heard of the misfortune, which had befallen his father; but departed soon after, leaving behind him *Saloninus* his elder son, whom he created *Caesar*; and placed with him a tribune, by *Zosimus* called *Silvanus* (4), but *Zonaras* calls him *Albanus* (5). This officer being jealous of *Postumus*, made it his business to disgust him, break his measures, and render him suspected. Thus when *Postumus*, after an obstinate engagement, had intirely defeated several detachments of the *Brueteri* and *Chamavi*, people of *Germany*, who had passed the *Rhine*, and plundered the neighbouring country, he gave the spoil to his soldiers. But *Silvanus* wrote him a severe letter, and in the name of *Saloninus* ordered him to deliver

(1) M. CASSIUS LATIENVS POSTVMVS.

(2) *Tréb. Pollio*, in *Postumo*.

(3) *Flav. Vopiscus*, in *Aureliano*.

(4) *Lib. i.*

(5) *Tom. i. pag. 632.*

deliver up the whole booty, the disposal of which was claimed by the prince. The army upon hearing of this order assembled in a tumultuous manner, declared both *Gallienus* and his son unworthy of the empire, tore off their images from the military ensigns, and obliged *Postumus* to assume the purple for the security of his own life (1). After this they marched immediately to *Cologne*, where *Saloninus* and *Silvanus* then resided; and compelling the inhabitants to give them up, they massacred them both.

*Gallienus*, more concerned for the loss of his son than his father's imprisonment, sent no forces against the *Persians*; but ordered his best troops to march into *Gaul*, with a shew of determined vengeance. *Postumus* opposed them for three whole years, at the end of which he was quite exhausted, and obliged to defend his last town himself; when *Gallienus*, who was desirous of the honour of conquering him in person, approaching too near the wall, was wounded in the shoulder with an arrow; which caused him to raise the siege, and return to *Rome*, without thinking any more either of *Postumus* or the *Gauls*. Who being thus freed from the danger which threatened them, justice, plenty, and the love of arts, flourished among them under the government of *Postumus*; to whose honor divers monuments were erected, representing as their tutelar deities, the goddess *Pax*, but principally *Hercules*. And several of his medals are yet extant, with the title, *RESTITUTOR GALLIARVM* (2).

But

(1) *Zonaras*, ubi supra. *Treh. Pollio*, in *Postumo*.

(2) *Cabinet du Roy*.

But this felicity did not long continue ; for the *Germans* and their confederates, whom he had defeated near *Cologne*, instigated by *Lollianus* (or rather *L. Aelianus*) who had been raised to oppose him, made a new eruption on the side of *Mayence* ; where he again defeated them, drove them into the town, and so straitened them, that they offered to surrender. *Postumus* gave his consent, but the army would not ; and rather than lose the plunder of a single town, sacrificed their general, whom they had themselves advanced to the imperial dignity.

Mr. *de Boze* places the death of *Postumus* in the spring of the year 267, being the seventh of his reign, and about the seventieth of his age. And his son of the same name, who is sometimes joined with him upon medals, appears to have lived near forty years. *Valerian* had given him the government of the *Vocantii*, a people in *Dauphiny*, at the same time that he sent his father to command in *Belgic Gaul*. And he was so considerable an orator, that some have ascribed to him the nineteen *Declamations*, which others assign to *Quintilian*, the grandfather of him, whose excellent treatise intitled, *Institutiones Oratoriae*, are yet preserved, and to which those *Declamations* are usually subjoined (1). *Trebellius Pollio* says, that both the *Postumi* were killed in the sedition at *Mayence*. But Mr. *de Boze* doubts the truth of this, and thinks the credit of so inaccurate a historian, who mistakes even in their name, which he always writes

R *Posthumius*,

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(1) *Treb. Pollio*, in *Postumo juniore*. *Berchii* *Advers. Lib.* LVIII. c. 14. *Voss. Instit. Orat. Lib.* I. c. XI.

*Posthumus*, not sufficient to support it. He rather thinks therefore, that the son died some years before his father, and probably during the war, in which they were jointly engaged against *Gallienus*. For it was immediately after this war, that his father took *Victorinus* (1) as a partner with himself in the empire; which, had his son been living, could neither have been for his interest, nor suitable to natural affection. Besides, tho *Victorinus* had then distinguished himself for his valour, he must have been younger than the son of *Postumus*; since his mother *Victorina*, or *Victoria*, was yet in the vigour of her age, and one of those who opposed *Gallienus*. And tho she could not possess the empire herself, such was her ambition, that she assumed the titles of *Augusta* and *Mater castrorum* (2).

Her son *Victorinus*, who expected to succeed to the government of *Postumus*, finding his army joined to the faction of *Aelianus*, and that he was proclaimed emperor, had no small difficulty to support himself among the other part of the *Gauls*, who by this means were divided. But this did not remain long; for *Aelianus* having undertaken to repair the forts, which *Postumus* built along the *Rhine* to prevent the passage of the barbarians over it; and they  
had

(1) M. AURELIUS PIACVONIUS VICTORINUS.

(2) The title *Augusta* was usually given to the mothers and wives of the emperors; and *Trajan* conferred it likewise on *Marciانا* his sister, and his niece *Matidia*. The other title, *Mater castrorum*, took its rise from the repeated acclamations of the armies; and had been given to *Faustina* the wife of *M. Aurelius*, and *Julia Domna* the wife of *Septimius Severus*, with some others.

had indeavoured to demolish, after they heard of his death; he imployed all his forces in that work, who displeased with the labour revolted and slew him (1). His death restored to *Victorinus*, what he had lost by that of *Postumus*, for he was acknowledged by all the *Gauls*. But he was soon after taken off by a private conspiracy, for an intrigue with the wife of a notary or clerk (2). His son also was killed with him, whom he had created *Caesar*, tho but an infant (3).

The army soon fixed on a successor, and imagining they should find better treatment from an inferior officer, with whom they had been more familiar, chose one *Marius*, an armorer by trade, who had raised himself among them, and was greatly admired for his intrepidity and bodily strength, qualities of the highest esteem with the vulgar. But *Marius*, thus advanced to the imperial dignity, immediately behaved with that cruelty and insolence, that one of his old friends, trained up in his business, and afterwards his companion in the army, resenting his arrogant behaviour towards him after his advancement, stabbed him privately, with this opprobrious insult: *Hic est gladius, quem ipse fecisti* (4). His government was very short; *Aurelius Victor* (5) and *Eutropius* (6) make it no longer than two days, but *Trebellius*

R 2

*Pollio*

- (1) *Treb. Pollio*, in Lolliano.
- (2) *Aurel. Victor*, De Caesar. c. 33.
- (3) *Treb. Pollio*, in Victorino.
- (4) *Idem*, in Mario.
- (5) De Caesar. c. 3.
- (6) Lib. ix. c. 7.

*Pollio* fais three (1). Tho Mr. *de Boze* justly observes, that neither of these accounts agrees with the number and variety of his medals, which seem to require more months; for they equal those of his predecessor *Victorinus*, who reigned three years, from the time of his being an associate with *Postumus* in the empire. Besides, some of his medals appear to have been struck on the account of victories gained by his lieutenants in that part of *Britain*, which was subject to the emperors of *Gaul*; the truth of which victories is confirmed by other monuments and inscriptions found in that country (2), which seems no ways consistent with so short a reign. From whence he thinks it reasonable to allow him four or five months, to be reckoned from September or October 267, to the end of January or February 268.

His successor *Tetricus* (3), the principal subject of this discourse, was a senator, had discharged the office of consul, successively governed almost all the *Gallic* provinces, and was then prefect of both the *Aquitains* (4). He was related to *Victorina*, who caused him to be declared emperor in his absence, and the title of *Caesar* to be given to his son. And being solemnly invested with the imperial dignity at *Bordeaux*, he was acknowledged, as *Postumus* had been before him, by all the *Gauls*, as likewise part of *Spain* and *Britain*. The time of his advancement

(1) *In Mario.*

(2) *Gul. Malmesburiensis, De rebus gestis Reg. Angl. Lib. ii. Camden. Britannia, p. 641. edit. 1607.*

(3) *PVB. PIVESVS TETRICVS.*

(4) *Treb. Pollio, in Tetrico seniore.*

ment to the empire Mr. *de Boze* places between the end of January and part of March, in the year 268 (1), agreeably to the time he had assigned for the reign of *Marius*. For he observes, that all historians agree, that *Gallienus* was yet living, when *Tetricus* was declared emperor among the *Gauls*; and doubts only, whether he had heard the news, when he was killed before *Milan*, where *Aureolus* was besieged by him. His death therefore must have happened about the eighteenth or twentieth of March at furthest that year. For his army having in the field elected *Claudius* the second, afterwards surnamed *Gothicus*, of which he sent an account to the senate, the letters arrived at *Rome* on the twenty-fourth; and the senate being convened the same day in the temple of *Apollo*, among other acclamations these following were several times repeated: *Claudi Auguste, tu nos ab Aureolo vindica. Claudius Auguste, tu nos a Palmyrenis vindica. Claudius Auguste, tu nos a Zenobia et a Victoria libera. Claudius Auguste, Tetricus nihil fuit, or nihil sit, or nihil fecit* (2), as the words are read in different manuscripts. From hence therefore he infers, that *Tetricus* must at that time actually have held somewhat, which could be nothing else but the empire of the *Gauls*.

The reign of *Claudius* was not long enough to answer all the hopes, which had been conceived of it. He marched immediately against *Aureolus*, and defeated him. After this he purposed to turn his arms against  
*Zenobia,*

(1) V. Rom. 1021.

(2) *Treb. Pollio, in Divo Claudio.*

*Zenobia*, queen of *Palmyra*; but a deluge of *Goths* having on a sudden overspread *Sylyrium*, *Thrace*, and *Macedonia*, he determined to repel them. And being obliged to take all his forces with him in that expedition, he not only forbore himself to disturb *Tetricus* in his possession of the *Gauls*; but likewise, as Mr. *de Boze* very probably conjectures, gave orders, that nothing might be done in his absence to interrupt the peace, they had enjoyed since the retreat of *Gallienus*. This real, or at least feigned freindship, between these two emperors seems to be intimated by medals, struck with the head of each of them on the opposite sides (1). And the like, as he very ingeniously remarks, may be gathered from a passage of *Eumenius*, in his *Panegyric* upon the emperor *Constantine*, where addressing to him in behalf of the *Aedui* he says: *Deum Claudium, parentem tuum, ad recuperandas Gallias primi sollicitaverunt; expectantesque ejus auxilium septem mensibus clausi, et omnia inopiae miseranda perpeffi, tum demum irrumpendas rebellibus Gallicanis portas reliquerunt, cum fessi observare non possent* (2). These *Aedui*, who applied to *Claudius* for his assistance, opposed at that time the government of *Tetricus*; whose medals give us a more noble idea of him, from the use he made of his victories. For in some of these he is represented not as a warrior, but in a state of peace and plenty, with the legend *SALVS AVGVSTORVM*, intimating that moderation in success is the true grandeur

(1) *Ansel. Bandur. Numism. imper. Tom. I. p. 403.*

(2) *Edit. ad usum Delphin. pag. 222.*



grandeur and safety of princes. And in others are seen the figures of several temples erected by him, some of them in a circular form like the *Pantheon*, with the legend *PACI*.

And to these happy times Mr. *de Boze* refers the curious gold medallion mentioned above, which represents *Tetricus* as crowned with laurel, and dressed in the *toga palmata*, or consular robe, which was also worn in triumphs. In his right hand he holds an olive branch, and a scepter with the Roman eagle on the top in his left, and round his image is this inscription, *IMPERATOR TETRICVS AVGVSTVS*. But the reverse, if it has one, must remain unknown; since the medal is so fixed in the gold box, which contains it, that they cannot be separated without endangering both. And the radiated circle of gold, with which it is encompassed, is designed only to adorn and enlarge it.

He therefore proceeds with the history, and further observes, that *Claudius* gained a most signal victory over the *Goths*, in which three hundred and twenty thousand of them were slain, and two thousand transport vessels taken. But as this victory had been greatly facilitated by a pestilence, which spread thro the enemies army, it seized likewise the Roman forces, and *Claudius* himself died of it at *Sirmium* in *Pannonia*, in the third year of his reign, and fifty-sixth of his age (1).

His

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(1) *Zosimus*, Lib. i. c. 1. *Deinceps*: *De locutionibus Porphyrianis*, in Vit. Plotini, Basil, 1591

His brother *Quintillus*, who had been left with some forces in *Italy*, was proclaimed emperor there, and acknowledged by the senate. But being informed, that the victorious army had elected *Aurelian*, and perceiving that his own forces were inclined to abandon him, he chose to free himself from those unhappy circumstances by a voluntary death (1).

*Aurelian* was not forgetful of the war, which his predecessor *Claudius* had designed against *Zenobia*, whose power daily grew more formidable, as she had then got possession of *Aegypt*. However the difficulty of the enterprize, and the great preparations necessary for undertaking it, occasioned him to defer it near two years. But the vigorous manner, in which it was afterwards carried on by him, and the great success that attended it, are too well known to need a recital here. In the mean while *Tetricus*, who remained unmolested, was constantly employed in studying the welfare and prosperity of the *Gauls*. And as both the situation, and natural fertility, of the country are very well suited to promote commerce; those advantages were greatly improved by him, as well by repairing the roads, as making new ones, the oversight of which works was committed to his son; some proofs whereof yet appear from inscriptions on the milliary pillars, erected to mark out the distance of the ways (2). The legends also upon some of their coins, struck in honor of *Tetricus*,  
plainly

(1) *Zosimus*, Lib. 1. *Zonaras*, Annal. Tom. 1. p. 636.

(2) One of these inscriptions is yet preserved at *Rouen*, in the house of Mr. *Bigot*.

plainly exprefs the happiness, which the *Gauls* enjoyed under his auspicious government; such as *VBERTAS*, *LAETITIA*, *FELICITAS PVBLICA*, and the like (1).

*Victorina* did not long survive the advancement of *Tetricus* to the empire of the *Gauls*; but whether she died a natural death, or was killed, historians have left quite uncertain (2). However, the arts of intriguing and caballing, which she had carried to the greatest hight in *Gaul*, did not die with her; but gave *Tetricus* a continual uneasiness, either to detect or suppress them. And therefore upon the return of *Aurelian* from the conquest of *Zenobia*, whom with her two sons he sent to *Rome* in great pomp; when *Tetricus* could no longer bear with the insolence of his own soldiers, he wrote a letter to him, in which he used this expression: *Eripe me his, invicte, malis* (3). And afterwards upon the arrival of *Aurelian* near *Chalons* in *Campania*, drawing out his forces, as if he designed an engagement, he surrendered to him both himself and his whole army. By this means *Aurelian* being then, as the historian expresses it, *princeps totius orbis* (4), celebrated a most splendid triumph at *Rome*; in which not only *Zenobia* with her two sons, but likewise *Tetricus* and his son, were exposed to public view among the other captives, to denote the subjection both of the eastern and western empire.

Some

(1) *Cabinet du Roy.*

(2) *Treb. Pollio, in Victorina.*

(3) *Idem, in Tetrico seniore. Eutropius, Lib. ix. c. 19.*

(4) *Flav. Vopiscus, in Aureliano.*

Some authors blame *Aurelian* for treating *Tetricus* in so indecent a manner; which however he is supposed to have done with a view to humble the senate, against whom he was greatly prejudiced; and therefore chose thus to show himself (as he was usually called by the populace) *senatorum paedagogum* (1). For *Trebellius Pollio* informs us, that he afterwards treated *Tetricus* with the highest honor, often calling him colleague, sometimes fellow soldier, and at other times giving him the title of emperor (2). His estate also was restored to him, and his house, which had been demolished, was rebuilt on mount *Coelius*, changed into a palace, and dedicated with solemnities like a temple. *Aurelian* was himself invited to this ceremony, and having entered the grand hall, was surprised to see himself represented there, as delivering to *Tetricus* and his son the senators robe with other marks of dignity, and receiving from them a civic crown and scepter (3). *Casaubon* finding that one of his manuscripts had not the word *sceptrum* in this passage of *Trebellius Pollio*, and not perceiving any reason for its being mentioned, proposes in his comment the omission of it. But Mr. *de Boze* very justly observes, that the gold medallion of *Tetricus* is the most certain commentary upon the place; where the scepter represents that of the Gallic empire, which compleated the glory and happiness of *Aurelian*.

Wherefore:

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(1) *Flav. Vopisc. in Aureliano.*

(2) *Treb. Pollio, in Tetrico seniore.*

(3) *Idem, in Tetrico juniore.*

Wherefore as he now thought himself in a condition to avenge the outrages, committed by the *Persians* under *Sapor* upon the Roman empire, he entered upon that expedition; leaving the government of the greatest part of *Italy* to the care of *Tetricus*, with this complaisant expression: *Sublimius habendum regere aliquam Italiae partem, quum trans Alpes regnare* (1). But *Aurelian* had marched no farther than *Caenopbrurium* (a station in *Thrace* between *Heraclea* and *Byzantium*) when he was killed by the treachery of his secretary, by some called *Mnestheus* (2), and by others *Eros* (3). Who from a fear of punishment, for some misdemeanours committed by him, had drawn up a list, containing the names of many persons, some of whom had incurred the emperor's displeasure, and others not; among which names, to prevent suspicion, he had inserted his own. This list, which appeared as written with the emperor's own hand, he shewed to the several persons named in it; adding withal, that *Aurelian*, whose temper was implacable, had determined to take them all off, if they did not prevent it in time. The scheme took effect, and *Aurelian* was assassinated in his march. But this treachery was not long concealed; and the army to avoid a fresh mistake referred the choice of a new emperor to the senate, who returned them the compliment; and after several reciprocal messages of that sort, which occasioned an interregnum of seven  
or

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(1) *Aurel. Victor*, in *Epitome*. *Treb. Pollio*, in *Tetrico seniore*. *Eutropius*, Lib. ix. c. 9.

(2) *Flav. Vopiscus*, in *Aureliano*.

(3) *Zosimus*, Lib. i.

or eight months, the senate elected *Tacitus* (1), one of their own body, and who was at that time *princeps senatus*.

No historian has settled the time, when *Tetricus* died. For *Zosimus* only tells us, either from vulgar reports, or mistaken memoirs, that he was put to death by *Aurelian* among other rebels in *Gaul* (2). All the rest agree in saying, or suggesting at least, that he survived his resignation of that empire; which however could not be more than about eighteen months, or two years at the farthest, if he died before *Aurelian*. But that does not suit with the medals of *Tetricus*. For some of these have his head on one side, with a radiated crown; and on the other an eagle, funeral pile, or flaming altar; with the legend *CONSECRATIO*, the usual emblem of deification. And as it can scarce be thought, that a prince of *Aurelian's* martial disposition would permit divine honors to be conferred on one, whom he had himself led in triumph to mortify the senate; it is more natural to suppose, that it must have been done by *Tacitus*, who was raised to the empire by the senate, thought it his honor to govern by their counsels, and had nothing more at heart, than to repair the injuries they had suffered by his predecessors. And this opinion seems further confirmed by considering, that the successors of *Tacitus* had a military turn, and copied after *Aurelian* both in their sentiments and actions. As *Tacitus* therefore reigned but about seven months, from September

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(1) MARCVS CLAVDIVS TACITVS.

(2) Lib. I.

275 to some part of March 276, Mr. *de Boze* places the death of *Tetricus*, and his consecration consequent upon it, within that time, as what appears to him the most probable. And with that event he concludes his elaborate and curious discourse.

G. C. March 21, "  
1753.

J. Ward.

XXI. *An Account of a Treatise, presented to the Royal Society, intitled, Flora Sibirica, five historia plantarum Sibiriae tomus secundus, extracted and translated from the Latin of Professor Gmelin, by W. Watson, F. R. S.*

Read April 12, 1753. **T**HIS volume of the *Flora Sibirica*, now under consideration, contains two hundred and forty pages in quarto, exclusive of the preface, and ninety-eight copper plates very curiously engraved. It was printed at Petersburg in the year 1749.

An account of the first volume of this valuable work was communicated to the Society by my learned and ingenious friend Dr. John Fothergill\*, and has been published in the *Philosophical Transactions*: from its title, we are only promised an account of the plants of Sibiria; but Dr. John George Gmelin, its author  
at

\* See *Phil. Transf.* Vol. XLV, pag. 248.

at that time professor of chemistry and natural history at Petersburg, and now at Tübingen, went much farther, and has given us a very great number of new, curious, and useful observations, concerning the natural history of that vast and unknown region. The abundance of matter, and the limits of an extract, obliged Dr. Fothergill to confine himself, principally to the geographical and meteorological part of the work; but as the contents of the second volume are chiefly botanical, I find myself obliged to take a review of the first volume, to introduce with propriety an account of the contents of the second.

The *Flora Sibirica* contains the plants, which grow spontaneously in a region of vast extent, bounded by the Vralensian mountains on the west, the ocean of Kamtschatka on the east, the *Mare glaciale* on the north, the countries of Kalmucks and Mongales, and the confines of China on the south. Our author has, among the productions of these countries, interspersed a few plants, collected by that excellent botanist Gerber, near the rivers Don and Wolga, and in the Ukraine; partly because many of the same kind grow in Sibiria, and partly from a desire that these curious plants should no longer be concealed from the public. He has given no plant a place, which he himself hath not examined, at least in a dried state; and of which he is not satisfied of its generical character.

The plants of Kamtschatka were collected by two of their company, detached for that purpose. Of these George William Steller is mentioned by our author, with very great respect, for his uncommon zeal and proficiency



proficiency in natural history; and for his offering himself to go upon an expedition, where he must for a long time, in very unhospitable regions, not only forego all the accommodations of human life, but be frequently liable to the miseries arising from hunger, cold, and the barbarous inhabitants. Of these dangers, to use our author's own words, he was *contemptor strenuus*, and continued several years upon the expedition, sending from time to time to our author large collections and descriptions of such natural bodies, as occurred to him. This excellent person, to the great grief of our author, and to the irretrievable loss of natural history, died on his return home, after having surmounted almost incredible difficulties.

In digesting the plants of the work before us into classes, our author has followed the method of our worthy brother Professor Van Royen of Leyden, published in the *Flora Leydensis prodromus*. This excellent botanist considers, that all plants may be ranged into twenty classes; and our author, in consequence of this system, has given five classes in his first volume, *viz.* those which Van Royen intitles, *Palmae*, *Lilia*, *Gramina*, *Amentaceae*, and *Umbelliferae*; and three classes in the second volume, *viz.* *Compositae*, *Aggregatae*, and *Tricoccae*: the twelve remaining classes therefore are, I presume, to be published hereafter. Our author follows Van Royen's system, not as he thinks it a perfect one, but as it nearly approaches to the order of nature; and has great relation to our countryman Mr. Ray's method, to which our author had been accustomed; and differs from it chiefly, inasmuch as it is more complete. Some allowance must be made for custom. He does not detract

detract from the methods of other authors ; most of them please him ; none of them are without their inconveniencies, and all have their advantages. The great point is, that the species should be well defined, and that each genus should have its essential character. Our author has generally adopted the genera of Linnæus ; some indeed he has taken from Haller ; but wherever he thought it expedient to differ from these great men, he gives his reason ; and when he finds a plant, which cannot properly be ranged under any genus already established, he forms a new one ; in the explanation of which, after the manner of Linnæus, he omits nothing essential thereto.

To the different species, discover'd in this expedition, P. Gmelin has affixed names, after the manner of Linnæus, Haller, Van Royen, and the more modern botanists, which are such, as that from the name of the species the plant may be known. But in what relates to the plants before discover'd, he adopts the names given them by the botanists just now mentioned, and scarce ever forms a new one ; as he thinks a name already received, though but an indifferent one, should be retained, in preference perhaps to a better ; lest the number of synonyms, already too great, should be augmented. To these he usually adds the synonyms of the Bauhins and Tournefort ; and sometimes, for the sake of their figures, those of Morrifon, Dodonæus, Plukenet, and Lœselius ; and likewise those of the Russian botanists, Messerschmid, Bauxbaum, and Amman. He has also throughout the work carefully separated the varieties of plants from their genuine species, and has laid down the places of their growth, the names given them by the inhabitants, and

and their application of them to the various purposes of life. The figures of the plants were taken from the life, and are, as far as possible, represented in their natural proportion; but from these must be excepted those of Gerber, collected near the Don and the Wolga, and some others collected by Dr. Lerche, physician to the Russian embassy in Persia, near Astracan, and even in Persia; these were delineated from dried specimens: and where-ever the figure does not, to our author's satisfaction, represent the plant intended, by the neglect of the painter or engraver, he apprises you thereof, and endeavours to remedy this defect in his descriptions.

The great end of our knowledge in plants should be the investigation of their properties; and to this we are frequently obliged to be led, by the application of them among the people, where they are produced. In perusing therefore the *Flora Sibirica*, I have selected a few observations of this kind, which I think not improper to lay before the Society.

The venereal disease has made no inconsiderable progress among barbarous, as well as among the more polite and civilized nations; and our author has given us two methods of treating that distemper among the inhabitants of Sibiria; from which, in some degree, an idea may be formed of the state of medicine in those parts of the world. One method is, they give the patient a decoction of a species of *cirsium* \*, which

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\* “ *Cirsium inerme* foliis scabris, lanceolatis, inferioribus ex  
 “ sinuato dentatis, squamis superioribus calicum subrotundis, mem-  
 “ branaceis.” *Flor. Sibir. Tom. II. pag. 72.*

which grows in those parts, and is described by nobody before our author: in this decoction, when the pains are violent, they add some leaves of a species of *chamærhododendron*, which produces effects similar to opium, by relieving the pain, and sometimes bringing on a delirium. If they are not cured by this decoction, which often happens in an aggravated state of the disease, they then boil a small quantity of sublimate of mercury, with some fat, in a spoon over a candle, mix it with the before-mentioned decoction, and let the patient swallow it. It is no wonder, if, according to our author's relation, this rude method should destroy the patient, and put an end to his life by severe torture, which frequently happens.

The other method of cure given us by our author is a more reasonable one, and is effected by administering a cup-full or two of the decoction of a species of iris \* every morning, detaining the patient in bed. Of this they give a greater or less dose in proportion to its operation, which is both by vomit and stool. After having taken it a week, it ceases to have the effect of evacuating; nevertheless they continue it another week; during which time the patient is laid upon a heap of fresh burdock-leaves, and his body is also covered with these leaves, which must be renewed every day. This method is said to cure the disease radically.

Russians, Tartars, and other nations in these parts, eat as food, either boiled in milk, or roasted in the embers,

\* *Iris foliis linearibus, corollis imberbibus; fructu trigono, canaliculato. Lin. Hort. Cliffort. p. 19. Flor. Sibir. Tom. I. p. 27.*

*Iris pratensis angustifolia, non fœtida, altior. C. B. P. p. 32.*

embers, various species of the roots of lilies. The Tartars collect and dry the roots of the *dens canis* \* of the botanists, and boil them either with milk or broth, and consider them as very nutritious food. This root certainly is in every respect nearly related to Salep.

The Siberian hunters, who kill various animals for their fur, are obliged to go in search of them into the most desert parts of the country, and remain there during their dreadful winters. It happens often, that, from the intenseness of the cold, the leaven, which ferments their bread, is spoiled, and ceases to be of use. In this case they collect the inner bark of the larch-tree, which is very juicy and sweet, and cut it into small pieces, and digest it over the fire in warm water. They then add thereto some rye-flour, bury the whole in the snow, and let it remain there twelve hours; in which time the fermentation begins, and the fæces, which fall to the bottom, made excellent leaven.

Both the Russians and the people of Kamtschatka make great use of the § *sphondylium vulgare hirsutum*, of Caspar Bauhin and Tournefort; or, what we usually call, cow-parshnep. According to our author, the plant in question differs in nothing from that species, very frequently met with in the meadows and pastures both of Germany and England, but in its being much larger. This difference of size the Russian kind constantly preserves,

\* *Erythronium*. *Linnaei Hort. Cliff.* p. 119. *Flor. Sibirica*. Tom. I. p. 39.

§ *Heracleum foliolis pinnatifidis*. *Lin. Hort. Cliff.* p. 103. *Flor. Sibir.* Tom. I. p. 213. *Sphondylium*. *Rivini*. Tab. IV.

preserves, when planted in the botanic garden. What we generally meet with here in England seldom grows higher than three feet ; whereas the Siberian plant is double that size. Our author has given us a very exact description of it.

This plant, which has never yet been applied to any useful purpose in these parts of the world, is of very great importance to the Russians and people of Kamtschatka. They indeed apply it to very different uses ; the former distil their brandy \* from it ; the latter dry it to eat in winter. As these applications of this plant are, I believe, wholly new to us, and unobserved by any preceding author, I shall lay before you a short history of them.

About the beginning of July the radical leaves are arrived at their greatest size and perfection, of which only the footstalks are used ; though, as far as may be judged from the smell, the stem of the plant is equally valuable. These are stripped of their bark, and suspended in the sun in little bundles ; and as they grow dry, many of these bundles are tied together, and exposed again to the sun, until they have parted with all their humidity. They are afterwards put up in bags, and in a very short time are covered all over with a yellowish mealy saccharine exudation, of the flavour of liquorice ; which, if it is wanted, is shaken off, and used as sugar. The people of Kamtschatka never separate this substance from the stalks, but preserve them together, and eat them themselves, and regale their friends with them, as delicacies.

The

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\* Spiritum ardentem,

The Russians dry them in the same manner, in order for distillation, and infuse them in proper proportions with warm water, to which they add the berries of the \* mountain dwarf-cherry, or those of a species of *vaccinium* §, to promote fermentation. When this is over, they put both the stalks and the liquor, in which they have fermented, into a still, and draw off the spirit as usual. When the distillation is over, they do not throw away the stalks, until they have pressed out their juicy liquor, which is added to fresh stalks to promote their fermentation.

From this spirit first drawn, they by distillation draw off somewhat less than half its quantity, which is very like to rectified spirit of wine, and much more pleasant than corn-spirit.

It must be here observed, that, if either the stalks or leaves of this valuable plant are applied to the skin, they heat and ulcerate it. The people of Kamtschatka however eat the crude stalks, when stripped of their bark, in which their acrimony consists: But if, through ignorance, this bark is stripped off with their teeth, it inflames and vesicates their lips and gums, which will frequently continue a week before they are healed. In consequence of this, some have made the experiment of extracting a spirit from the stalks without stripping them of their bark; and they have found, that they have furnished an equal quantity of spirit with those, which have been stripped: But

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\* *Chamaecerasus montana fructu singulari cœruleo.* C. B. P. 451.

§ *Vaccinia nigra fructu majore.* *Parkins.* 1455. *Vitis idæa magna quibusdam.* *J. B. I.* 518.

But it has been observed, that those, who have drank of this spirit, have scarce escaped with life, and have complained violently of an oppression about the præcordia a long time after.

From the mealy substance, which exudes from the stalks of this plant, a spirit may be prepared; provided that this substance is diluted in a proper quantity of water, and made to ferment: but this is in much less quantity than from the stalks themselves. The fermented liquor likewise they use as wine, and frequently intoxicate themselves therewith. By what accident it was discovered, that this plant would by distillation furnish an inflammable spirit, and for many particulars relating thereto, I must refer you to the work itself; and our author informs us, that the several uses of this plant were collected with great labour by M. Kraskeninkoff, one of their company, from the Cossacks of Kamtschatka.

Dodonæus \* relates, that the inhabitants of Poland and Lithuania make themselves a kind of liquor, which the poor people use as beer, from the fermented leaves and seeds of the *Sphondylium*. This adds somewhat to the credibility of the former relation.

When Steller, whom our author always mentions with great esteem, was at Tobolski in the year 1738, he was informed, that two years before they were grievously afflicted there with pestilential carbuncles, which were of so contagious a nature, as to seize those, who approached the person affected. The disease first began in horses and oxen, and afterwards seized

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\* Dodon. Stirp. Histor. p. 304.



seized the human species. A red spot first was perceptible under the armpits, or in the thigh, attended with great itching; and in a few hours grew to a very large tumour, joined with a burning heat of the part affected: These symptoms were attended with a very acute fever, intire loss of strength, violent pains in the head, and redness of the eyes. An old country practitioner, famous in these parts for his judgment, cured persons labouring under this severe disease in a short time. He used first to the carbuncle the powder of an herb \*, of which is given a complete history and figure in this work, made into a thin pulvise with dregs § of beer: This pulvise, gently warmed, was applied to the part affected, and the patient confined to his bed, who was at liberty to take whatever nourishment he liked, except milk, brandy, or the flesh of pikes. During this time the patient drank plentifully of a decoction of this herb, collected during the time of its flowering; though the powder, applied as above, was prepared from the leaves, before the flower-stalk was produced. The carbuncle, from this treatment, did generally break in four-and-twenty hours, and the symptoms greatly abate. The wound was sprinkled with sal ammoniac, and healed in a short time. This disease affected the cattle in different manners; some suddenly set a running

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\* *Centaurea squamis ovatis, foliis pinnatis, foliolis decurrentibus, linearibus, serratis et integris. Flor. Sibir. Tom. II. p. 89. Tab. XLI.*

*Cyanus floridus odoratus Turcicus, seu orientalis major, flore luteo. Hort. Lugd. Bat. p. 211.*

§ *Fæce cerevisia*; though I am inclined to think yeast is intended, which is usually written *flos cerevisiæ*, or *fermentum cerevisiæ*.

running with all their swiftness possible, and continued so, till they dropp'd down dead : in others, carbuncles arose, which were dressed by the practitioner before-mentioned with the pultice just now described, mixing at the same time a large quantity of the herb with their food ; and by this method great numbers were cured. A plant so well recommended, and which will grow in our own country, deserves to be better known to us.

Thus much may suffice to lay before you some idea of the merit of this work ; throughout the whole of which the author has shewn a complete knowledge of the botanic science, among the first professors of which he is deservedly placed. He has given us the descriptions and figures of a great number of plants hitherto not described ; and it is to be hoped, that he will continue his diligence in publishing the remaining twelve classes. But he should not stop here ; it were to be wished, as so many skilful persons were engaged so long in this dangerous and expensive expedition, that their observations upon the remaining two kingdoms of nature should be communicated to the public ; as well as a complete history of their travels into these unknown parts of the world.

XXII. *A Letter to Mr. William Watson, F. R. S. from Mr. Philip Miller, F. R. S. concerning a Mistake of Professor Gmelin, concerning the Sphondylium vulgare hirsutum of Caspar Bauhin.*

Read May 3, 1753. **I**N the abstract of the *Flora Sibirica*, which you laid before the Royal Society at their last meeting, there was mentioned, that the inhabitants of Siberia eat the stalks of the *Sphondylium hirsutum* C. B. P. But I have great reason to believe Professor Gmelin has mistaken the species: For he describes that plant as growing upward of six feet high; whereas the common sort (as you well observed) seldom rises much above half that height. Therefore I am of the opinion, that the plant mentioned by Professor Gmelin is that species, which Doctor Breynius mentions, in his second *Prodromus*, under the title of *Sphondylium maximum Transilvanicum Ricini folio*; the seeds of which I brought from Dr. Boerhaave's garden in the year 1727, where it was growing by the common sort of Caspar Bauhin, and in the same soil and situation was more than twice the height: and the same has continued in the growth of both these plants since, in the Chelsea-Garden; where the large sort constantly rises to a stem, at least a month sooner in the spring than the common sort, and the leaves are much larger, less divided, and not so hairy; so that there can be no doubt of their being distinct species. The leaves of both sorts I have sent for your inspection.

The seeds of that species of Doctor Breynius I have received from Siberia, by the title of *Sphondylium vulgare*, and Doctor Boerhaave told me, he had received the seeds from Austria, Hungary, and Peterburgh, by the same name; so that it is certainly the common sort in those countries. And it is very usual to find many mistakes in the writers on botany; which has happened from their supposing, that the plants, which have been mentioned as common in one country, were the same with those of the country where they inhabited. An instance of this you well remember of the *Parietaria minor ocymi folio*. C. B. which is the only species found wild in England; and so was by all the English botanists taken for the *Parietaria officinarum* & *Dioscoridis* C. B. which are distinct species.

Many other instances might be mentioned of mistakes, which have arisen from the same supposition, were it necessary: but as you are so well acquainted with these things, so I shall not trouble you with the mention of any more at present, but remain,

S I R,

Chelsea, May 2,  
1753.

Your most obedient humble servant,

Philip Miller.

XXIII. *A Letter from the Rev. Mr. George Costard to Dr. Bevis, concerning an Eclipse mention'd by Xenophon.*

Dear Sir,

Read April 12,  
1753.

OF what use the doctrine of eclipses is in history and chronology, you know too well for me to tell you. The earliest account of any in the Greek history is that said to have been foretold by Thales to the Ionians. Of this I shall say nothing more at present, for fear of repeating what I have already observed to you upon that subject. The next, that is generally taken notice of by writers, is, that in the first year of the Peloponnesian war mentioned by Thucydides. But there is another before that, as I apprehend, equally remarkable, which, as Mr. Jackson (Vol. I. p. 426.) hath quoted me for, may deserve some farther consideration.

It is well known, that Herodotus, and other writers make Cyrus to have deposed Astyages. On the contrary, Xenophon says, that Astyages was succeeded by his son Cyaxares, who left the kingdom to Cyrus by will. The truth, I believe, is, that Cyrus did not depose Astyages, and therefore so far Xenophon is right, but deposed Cyaxares; in which he was designedly wrong. That he knew the Persians forced the empire from the Medes, I think, appears from some no very obscure hints even in the *Cyropædia* itself. But that argument I shall not enter into farther at present. In his *Anabasis*, which is nothing

but a journal of his march, and where he relates things as he found them, he expressly asserts it. For he says, that the Greeks in their return home, came down to the river Tigris, where had formerly been a large but at that time deserted city called Lariffa, inhabited formerly by the Medes. Ταύτην, continues he, βασιλεύς ὁ Περσῶν, ὅτε παρὰ Μήδων ΕΛΑΜΒΑΝΟΝ τὴν ἀρχὴν, παλιγορχῶν, ἐδενὶ τρόπῳ ἐδύναντο ἐλθεῖν. ΗΛΙΟΝ δὲ ΝΕΦΕΛΗ προκαλύψασα ἠφάνισε, μέχρις οἱ ἀνθρώποι ἐξέλιπον, καὶ ἔτας ἄλω.

Tho' Xenophon calls this a cloud, yet the word *νεφελή*, probably made use of by those, from whom he received his account, not only signifies a cloud, but any obstacle in general. That such effects could be owing to no common cloud, I imagined must be evident enough. And as the year before Christ 547, from a number of arguments, too long to be here insisted on, appeared to me to be the year, when Cyrus finished his reduction of the Median empire, I was naturally led to try, whether there was any solar eclipse that year, that could be the cause of so remarkable a darkness.

The geography of the east is so very imperfect, that it may be difficult to determine the situation of this Lariffa. For Xenophon hath given no other account of it, than that it lay on the banks of the Tigris. It is not improbable however, as Bochart thinks, that Xenophon inquired upon the spot, What ruins those were? And was answered *לרסן* *Laresen*, i. e. *Resen's*, or the ruins of *Resen*. But this he easily mistook, or changed into *Lariffa*, a name he was much better acquainted with.

Resen

Resen is a place mention'd *Gen. x. 12.* and there said to lie between Nineveh and Calah. This perhaps may help us in some sort towards its situation. *Mausil* (says Abulfeda) *metropolis est regionum Mesopotamiæ. Imposita est Tigri, a latere ejus occiduo. Ex adversum, a latere ejus eoo, est civitas Nineveh ruinis sepulta. Ad austrum Mausilæ effundit sese Zabrus minor in Tigrim.*

In Abulfeda, the longitude of Mausil is  $67^{\circ}$ , and its latitude north  $36^{\circ} 30'$ . But in Ulugh Beigh, and Nassir Ettusi, it hath longitude  $77^{\circ}$ . The last of these two authors agrees with Abulfeda, as to its latitude, but the former of them assigns it  $34^{\circ} 30'$ ; which makes a difference of two degrees, or 120 geographical miles. Chrysococca, in his tables, hath ΜΟΥΣΟΥΛΙΝ, to which he assigns longitude ( $\xi\theta$ ) 69, and latitude ( $\lambda\epsilon$ ) 35. This author reckons his longitudes ἀπὸ τῆς ἀκρας δυτικῆς θαλάττης, as Abulfeda doth his. But Nassir Ettusi and Ulugh Beigh begin theirs از جزایر خالدا from the Fortunate

Islands. The difference between the two computations is 10 degrees, according to Abulfeda, tho' others make it  $17^{\circ} 30'$ . Varenus, and modern geographers, place Mousof in longitude east from London  $43^{\circ}$ , and in latitude north  $34^{\circ} 32'$ : And this, I suppose, is as exact as any.

In the year before Christ, therefore, 547, the apparent time of the true conjunction was at Greenwich, October 21<sup>d</sup> 23<sup>h</sup> 39' 18".

The place of the luminaries  
Moon's latitude north  
Semidiameter of the earth's disk

°	'	"
6	24	9
		0
	46	24
	61	18
Semidiameter		

Semidiameter of the penumbra	33' 11"
Beginning of the central eclipse, 22 <sup>h</sup> 39' 7".	
End of the central eclipse 22 <sup>d</sup> 0 <sup>h</sup> 55' 55".	
Duration 2 <sup>h</sup> 16' 48".	
Angle of the moon's way with a circle of latitude, 84° 25'.	
Moon's semidiameter	16' 50".
Sun's semidiameter	16' 21".
Angle of the moon's way with a circle of latitude, 84° 25'.	
The sun rose centrally eclipsed, in longitude	0' 1"
west from Greenwich,	54 46 10
In latitude north	71 44 40
The sun set centrally eclipsed, in longitude	
east from Greenwich,	91 6 50
In latitude north,	23 9 40
The place of the center was at the following times, as computed at Greenwich, thus,	

	Times.				Longitudes.			Latitudes.		
	d	h	'	"	°	'	"	°	'	"
October	21	23	39	47	23	7	30E	39	48	40N
		23	47	31	28	36	40	34	20	50
	22	0	2	31	37	13	30	30	24	30
		0	17	31	57	19	40	26	56	40

By this table it is easy to see, that the center of the shadow crossed the deserts of Arabia, and then passed over the Persian Gulph below the mouth of the Euphrates. But if you will make the same allowance, as I did in my last, for the moon's acceleration,

or



or the small retardation of the earth's diurnal motion, the place of the center will be found at the following times, as reckoned at Greenwich, thus,

	Times.				Longitudes.			Latitudes.		
	d	h	'	"	°	'	"	°	'	"
October 21	23	2	31		39	1	40E	34	20	50N
	23	17	31		47	38	30	30	24	30
	23	32	31		68	9	40	26	56	40

By this table then it appears, that the center of the shadow passed more northward than the former track, and went over Kerkiffa, not improbably, the Carchemish of the prophet Jeremiah, and a small matter to the north of Bagdad. It is not improbable therefore, that it crossed the Tigris not far from the place where; it seems by Xenophon's account, Lariffa was situated, and where, consequently, it would cause such a darkness, as might well be attended with the effects he mentions.

This eclipse is, I take it, no inconsiderable acquisition to history and chronology, and is at the same time a confirmation of the suspicion, that, in these very antient ones, there is some allowance or other to be made for the influence of some cause, whatever it may be, hitherto not fully determined. This must be left for future observations. In the mean time however it may be of service to the science of astronomy, to examine all the past eclipses, that can be come at, and compare them with circumstances in the best manner we are able.

I have three more by me, which may some time or other make their appearance in the same manner

with the present one. In the mean time I shall add, that in Ebn Younis's eclipse, in the year after Christ 979, the time of the mean opposition was, by my calculation, May 14<sup>d</sup> 3<sup>h</sup> 50' 30".

	. 0 , "
The sun's place	1 27 53 0
The moon's place in the ecliptic	7 27 53 0
Moon's latitude north	37 42
Place of her apogee	4 13 50 46
Place of her node	2 5 4 14
Sun's mean anomaly	11 1 29 47
Place of his apogee	2 25 29 30
Digits eclipsed 8 <sup>d</sup> 4'.	

These numbers are something different from those, which, I think, I gave Dr. Bradley some time ago. For I had made, some how or other, a small mistake in the place of the sun's apogee, which was kindly hinted to me by Mr. M. I have, since that, gone over the whole calculation again, and believe it is pretty exact, and agrees well enough with what Ebn Younis relates, that *Spatium, quod eclipsatum fuit de diametro ejus (lunæ) fuit amplius quam octo digiti, et minus quam novem.* And afterwards, *Et erat luna, in hac eclipsi, in propinquo distantie suæ mediæ.*

I have been led insensibly farther than I intended at first, which was only to explain the grounds of what I had said to Mr. Jackson. But your love to the science will excuse the trouble I have given you, and make any farther apology needless. I am, dear Sir,

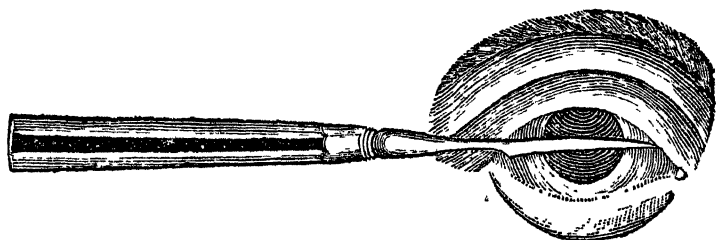
Oct. 3, 1752.

Your very affectionate, &c.

G. Costard.

XXIV.

XXIV. *A Description of a new Method of opening the Cornea, in order to extract the crySTALLINE Humour; by Mr. Samuel Sharp, Surgeon to Guy's Hospital, and F. R. S.*



Read April 12, 1753. **T**HE operation of discharging the crySTALLINE humour from the eye, for the cure of that species of blindness call'd a cataract, was a few years since invented by Monsr. Daviel, who has perform'd it on great numbers of patients, and continues still to practise it with remarkable success, as I have lately learned from unquestionable authority \*. Supposing it therefore admitted, that the extraction of the crySTALLINE humour has been found by experience to be a useful method of cure, I here take the liberty of laying before the Society a new manner of making the incision of the cornea, by which, I flatter myself, Monsr. Daviel's operation will be very much shorten'd, the patient will suffer less pain, and every skilful operator will be equal to the undertaking.

Place

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\* Mr. Morand, perpetual Secretary of the Academy of Surgery at Paris.

Place the patient in the same situation as for couching, either opening the eyelids with your fore-finger and thumb, or letting an assistant raise the upper eyelid, whilst you yourself keep down the under eyelid. Then, with a small knife, the figure of which is here represented, holding its edge downwards, make a puncture through the cornea near its circumference into the anterior chamber of the eye, in such a direction, as to carry it horizontally, and opposite to the transverse diameter of the pupil: after which you are to pass it towards the nose, through the cornea from within outwards, as near to its circumference, as in the first puncture.

When you have made the second puncture, push the extremity of the blade one seventh of an inch beyond the surface of the cornea, and immediately cut the cornea downwards, drawing the knife towards you as you make the incision. After this, you press gently with your thumb against the inferior part of the globe of the eye, in order to expel the cataract, and the operation finishes, according to the different circumstances, as in the manner proposed by Monsr. Daviel.

One extraordinary benefit seems to arise from the use of this single instrument, and perhaps from the shape of its blade, which increases in breadth all the way towards the handle: for, by this means, the punctures are so exactly fill'd up by the blade, that very little of the aqueous humour is discharged before you begin to make the incision, and consequently during this time, the cornea preserves its convexity; whereas by using one instrument to puncture, and others to dilate, the cornea immediately becomes

becomes flaccid, upon the issue of the aqueous humour, and renders the operation tedious and embarrassing, as I myself have found by experience in one patient, on whom I performed the incision of the cornea with a pair of scissars, as recommended by Monf. Daviel.

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XXV. *Experiments by Francis Hume, M. D.  
on Fish and Flesh preserved in Lime-water,  
communicated by John Clephane, M. D.  
F. R. S.*

Read May 3, 1753. **W**ITH a design to find out how long I could keep fish and flesh fit to eat in lime-water, I put two haddocks, and a pound of beef, in different pots full of lime-water, and corked them well. They stood in our cellar 18 days.

I then took out one of the fish: it was sweet, sound, and firm; I boiled one part of it, and I broiled the other: it eat well, and had not the least taste of lime-water; but was not just so firm as a fresh fish. But when I open'd the beef-pot, to my great surprize, it stunk abominably.

I poured the lime-water from both pots, and put in fresh lime-water. This stood 4 weeks longer; the remaining fish was quite fresh, and a little swelled, but, when I boil'd it, dissolved to a jelly. The flesh was very putrid.

Thus lime-water appears to preserve fish, but not flesh.

Dr. Alston's experiment was made with fish, and Dr. Pringle's with flesh ; which has made the former say, that lime-water withstood corruption strongly ; and the latter, that it did it but weakly, if at all.

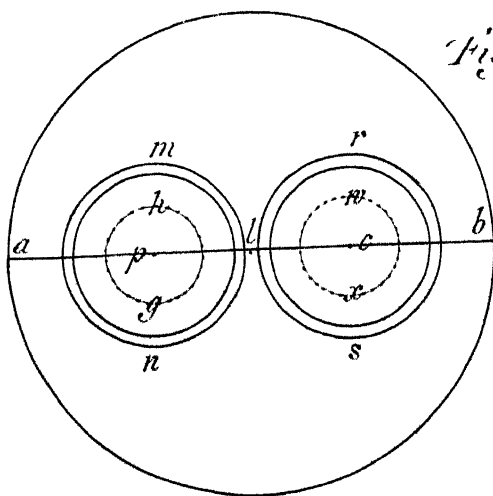
Edinburgh, April 6, 1753.

**I**LATELY repeated the experiment more fully, and with the same success. On the 26 of March I put a haddock into a pot of common water. I did the same to a piece of beef: the water was changed every day. At the same time I put a haddock into a pot of lime-water, and did the same with a piece of beef: at the same time I hung a fish and a bit of flesh in the air. On the second of April the fish and flesh in the air were a little corrupted and dried; the flesh and fish in common water smelt strong; the fish in the lime-water was sweet, and the lime-water good, and are so at present; but the flesh smelt rather worse than that in common water changed every day, and the corruption had quite overpower'd the smell of the lime-water.

All this you have my leave to shew, as a confirmation of the former experiment.

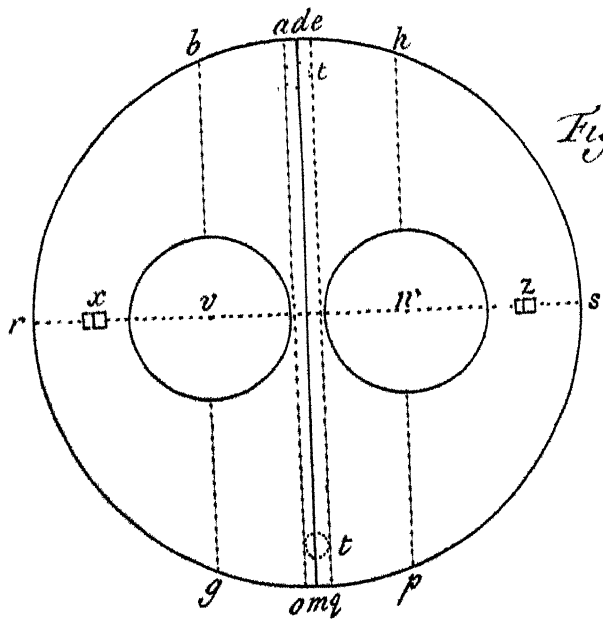


*Fig. 5.*

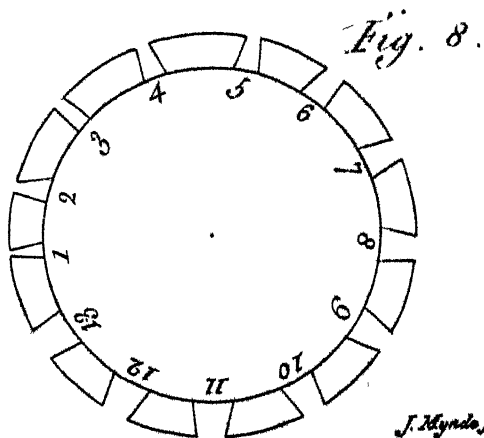
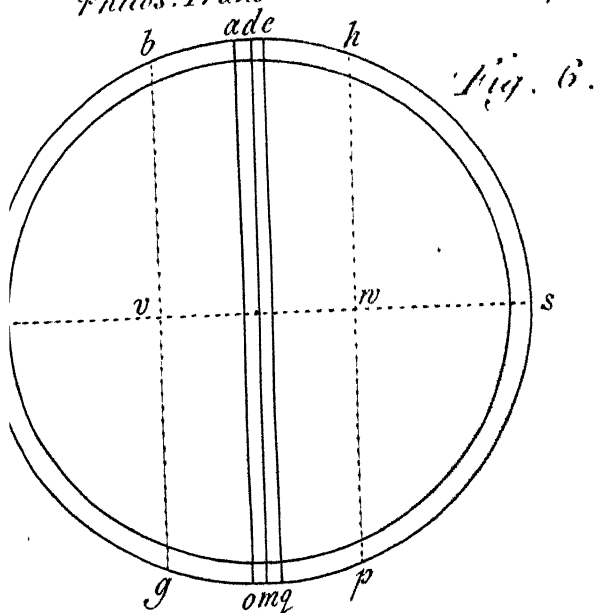


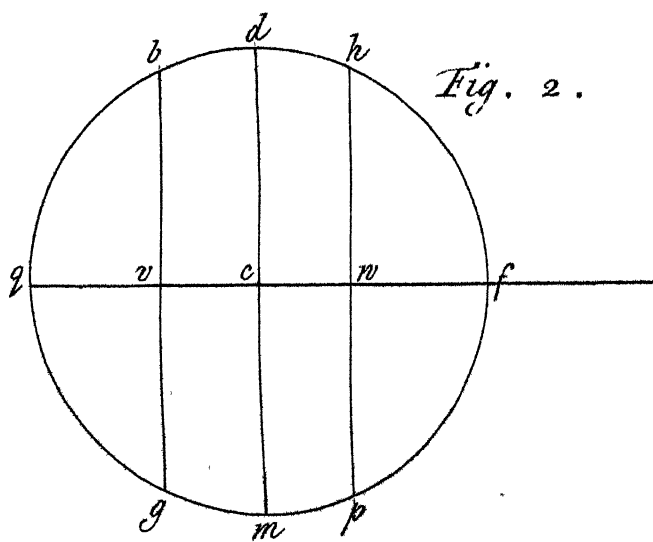
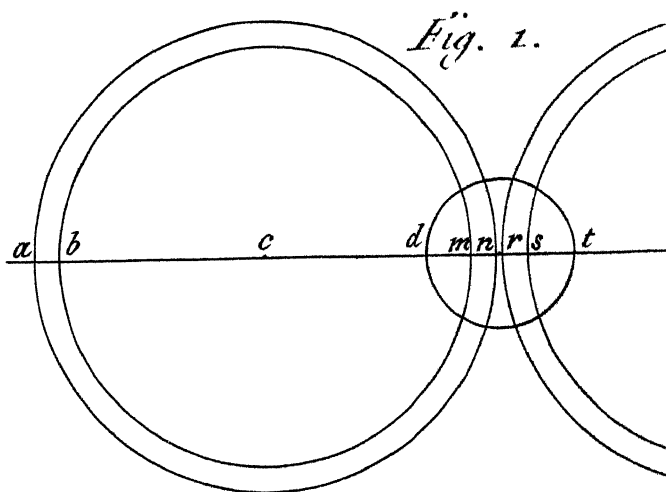
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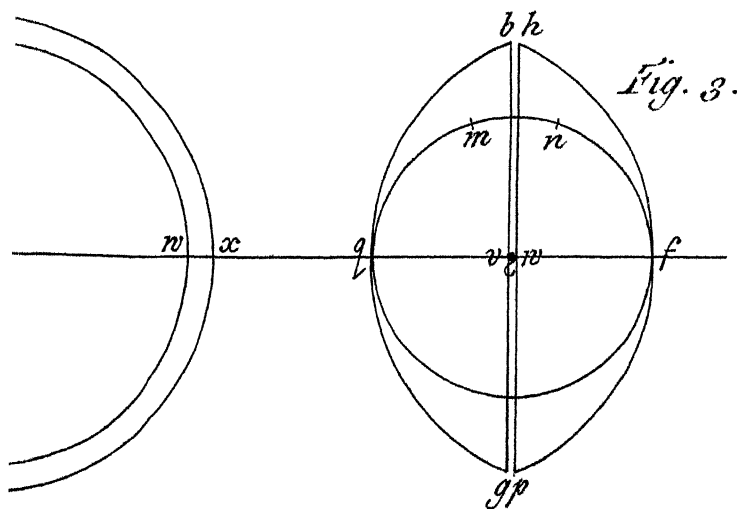
*Fig. 7.*



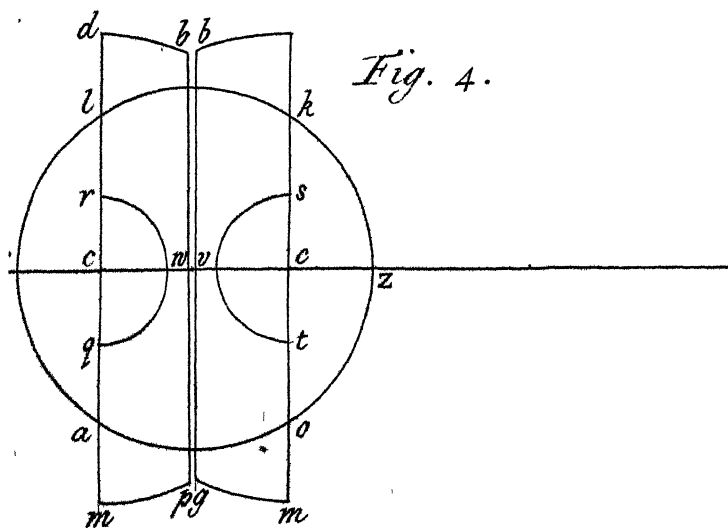








*Fig. 3.*



*Fig. 4.*



XXVI. *A Letter from Mr. James Short, F.R.S. to the right honourable the Earl of Macclesfield, President, concerning a Paper of the late Servington Savery, Esq; relating to his Invention of a new Micro-meter.*

My Lord,

Read May 10,  
1753.

**I**T is now above a year since I received a letter from the Rev. F. Pezenas, professor of hydrography to the French King at Marseilles, in which he informed me, that M. Bouguer had read, before the Royal Academy of Sciences at Paris, in the year 1748, a memoir, in which he describes an heliometer; which is an instrument, consisting of two objective glasses, for measuring the diameters of the planets. He said also, that this memoir was actually in the hands of M. de Fouchy, Perpetual Secretary of the Academy, or at the Royal Printing-house; and that it was register'd in the minutes of the Academy for the year 1748.

Immediately after reading this letter, I recollected to have heard a paper upon the same subject from the late Servington Savery, of Exeter, Esq; read before the Royal Society, about the year 1743. I therefore had recourse to the minute-book of the Society for that year, where I found the following minute, which I copied in the presence of the right honourable the Lord Charles Cavendish, then Vice-President:

“ A

I have therefore contrived some dioptric telescopes, and a reflecting one; either of which (by representing the object double) will, if well made, answer the design.

Fig. 1.

Represents the whole body of the sun, as it appears double, and magnified in the telescope. Let  $an$  be the diameter of the one, and  $rx$  of the other image of the sun *in perigæo*; so shall  $nr$  be the distance between the two images at that time; which measured with the micrometer is equal to (suppose) 10 seconds.

Let  $bm$  be the diameter of the one solar image, and  $sw$  of the other, when *in apogæo*: so shall  $ms$  be the then distance of the solar images, measuring with the micrometer (suppose) 1 minute 10 seconds. The difference of these two observations, 1 minute, is the apparent diminution of the sun's diameter.

The little circle, whose diameter is  $dt$ , is the whole area visible at once in the telescope, which is not one third part of the magnified diameter of the sun: but since both  $nr$  at one time, and  $ms$  at another time, are visible within the telescope's area, (if good instruments are procured) I can see no difficulty in performing what I have proposed above more accurately than it has ever yet been done, except this one (which some time since Mr. Graham in a letter to me mention'd) *viz.* that of defining the sun's disk truly: and I think to do that to good perfection, is beyond human art. A telescope for this use may be made to magnify the sun's diameter to any degree whatsoever, not exceeding such degree, as will make any part of the line  $ms$  fall without the area of the telescope: and I think it will be very difficult to make one with  
a charge

a charge so great, as not to have more than a geometrical minute of the sun's apparent diameter visible at once.

Since the sun is an object so very remote, the pencil of rays flowing from the center of its disk, and incident all over an object-lens (tho' it should be a foot broad) would not differ sensibly from a perfect cylinder within the distance of above 100 miles from its basis at the lens; tho' in reality the whole pencil is an acute cone, whose angle at the vertex is almost evanescent.

Hence it follows:

That if the two poles of two equal object-glasses are placed at the distance (suppose) of a foot from one another, the two centers,  $c$ ,  $v$ , of the two solar images must, as to sense, remain always at that very same distance (*viz.* 1 foot) from one another, tho' the sun should be placed ten times as far off as it now is: but since the sun's greater distance would diminish the diameters of both of the solar images;  $m n$ , added to  $rs$ , must be the true difference of the apparent diameters of the images (and also of the sun) at different times.

According to Mr. Azout (*Harris's Lexic. Techn. Vol. I. see SUN*), the apparent diameter of the sun never exceeds  $32' 45''$ ; whence its radius never exceeds  $16' 22' 30''$ ; the tangent whereof is about 476,328 (if I mistake not) to the radius 100,000,000.

As the said tangent : to the said radius :: so half an inch : to 104.96 inches, and decimal parts.

According to this,

If the focal length of a lens is 104.96 inches and parts, it cannot collect the sun's rays to a less focus

at the time of his perigee than one inch in diameter, or half an inch radius.

Fig. 2.

The whole circle represents a well-center'd object-lens, whose focal length is (as above calculated) 104.96 inches and parts (rather a little less, that the two images may be sure not to touch one another.) Let the two diameters  $dm$ ,  $qf$ , divide it into four quadrants, but the diameter  $qf$  must be occult, or delible. Let  $cw$  be half an inch, and  $cv$  equal to it. Through  $v$  (and also thro'  $w$ ) let a chord-line be drawn parallel to the diameter  $dm$ , viz.  $bg$ ,  $bp$ . Thro' the said chord-lines  $bg$ , and  $bp$ , and also thro' the diameter  $dm$ , divide the lens into four parts.

Fig. 3.

Let the strait edge of the frustum  $bvgq$  in the preceding figure be cemented fast to that of the similar frustum  $bwpf$  of the same lens, as they appear in this present Fig. 3. wherein, for the easier understanding the placing them, I have noted each frustum with the same letters it was noted withial in the preceding Fig. 2. Having then with barm fastened a white paper all over both sides of the lens I made for trial (which I did, not only to secure the cemented joint from breaking, but to prevent the injury which the polish might receive in cutting and grinding the edges) I describ'd a circle  $qmnf$  on the center  $c$ , fit for the tube I had to put in it; and having made it round, and wash'd it clean, after the edges were ground true, that nothing sandy might hurt the polish, I soak'd it in clean water, till I could easily take off  
the



the paper. I also, before I took off the paper, mark'd one end of each frustum in the edge at *m* and *n*, that if they fell asunder, they might be cemented together again the same way. This model (made of a spectacle-glass about 12 or 13 inches focus) gave me encouragement to try the following one, which I thought better.

Fig. 4.

I made my second model of the two middle frustums *mcdbwp*, *mcdbvg*, of the lens Fig. 2. by cementing their edges, *bwp*, *bvg*, together, as they are placed in the present Fig. 4. so the pole *c* of each part must consequently be half an inch (supposing its focal length is about 104 inches) from the middle where *c* stood in Fig. 2. viz. the pole of one frustum where *v*, and of the other where *w* now stands. I left open at each pole a semicircular aperture *rwq*, *svt*, about two thirds of an inch diameter, and cover'd all the rest of the circle *axlkzo*, to which I had cut it fit for the tube. The focus of the lens I made it of was about 3 feet.

Note, The rays of red light in the two solar images will be next to one another in both these models, which, I take it, will render the sun's disk more easy to be observed than the violet ones. This I mention, because the glasses in these two sorts are somewhat prismatical, but mostly those of my first model, which could therefore bear no great charge. Also the frustum on the right-hand of my first model renders the solar image at the focus on the left, and that on the left-hand renders it on the right: but it is not so with the second model, or with the next contrivance, which is the best, if well made.

Fig. 5.

In this the greatest difficulty consists in getting two well-center'd object-glasses, whose focal lengths are equal ; for it is necessary they should be so, because they are to be combined with the same convex eye-lens (common to them) at the same distance.  $a b$  is the diameter of a plain brass plate, which may be two inches and a half broad, or somewhat less ; two short equal cylindric brass tubes,  $m n$ ,  $r s$ , must be fastened thereon, with their centers  $p c$ , equidistant from the center  $l$  of the plate, and distant one inch from one another in the diameter  $a b$ , as the figure sheweth. In the tubes must be put two equal object-glasses of the focal length of  $104 \frac{2}{10}$  inches, or rather somewhat less, as aforesaid. Through the plate there must be made in the middle of each tube a round aperture, *viz.*  $b g$ ,  $w x$ , whose diameters must be proportion'd to the focal length of the eye-lens, and not exceed the third part thereof, lest the object appear confused.

And since it is scarce possible to center an object-lens to very good perfection, those in the two cylinders (tho' put in by a good artist) may happen to render the two solar images at too great a distance from, or too near to one another. But this fault, if not too great, may be remedied, by turning one or both of the lens's a little way round ; and then their eccentric poles will by that means be brought nearer to, or further from, one another ; and when they are once well plac'd, there should be a mark made in each lens, and its cylinder ; that if it is taken out to be wiped, it may be put in again the same way. There should also be a different mark in one of the

the glasses, that each may know its own cylinder. They must both of them be very close all round to their respective cylinders; otherwise one lens may slide nearer to or further from the other; which if it should in the least degree, between the first and second observation, all the labour would be lost.

Either of these three parts of double lens's may be combined with a convex eye-lens as usual, and have a micrometer placed at the common focus.

Such a double lens, of either sort, may be proved whether it is well composed or not, without the trouble of combining it with its eye-lens; by holding it in the sun's rays, as one would a burning glass, and applying a piece of white paper at its focus, where, I apprehend, the two solar images will appear as distinct as when an eye-lens is applied, tho' not so large; and each of them one inch broad, if the focal length is as above, *i. e.* almost  $104\frac{2}{5}$  inches.

After the same manner may the double object-mirror of a reflecting telescope for this use be proved.

### Fig. 6.

The circle *bdbpmg* is the circumference of a concave mirror made of black glass: it must be very thick, that it may not spring or bend with any thing that presseth on it to keep it fast; for that may injure the concavity of it.

The circle within it on the same center *c* sheweth, that the concavity thereof must not be continued quite home to the very edge of the mirror, but the little space between the two circles must be ground very true on a plain. The prick'd lines must not be drawn; they are only to indicate where the poles

$vw$  of the two frustums must be brought, after the mirror is diametrically bisected.

Let the concave side be defended, by pasting a paper all over it, and then let it be divided with a saw in the diameter  $dcm$ ; taking care that the said diameter be in the middle of the kerf, which may be as broad as the space between the lines  $ao$ ,  $eq$ . Let the asperities of the edges of both frustums be ground off, that they may be very strait after their being sawn.

Fig. 7.

Represents a thick round plate of brass very plain, and equally thick all over, having lines drawn on it, as on Fig. 2. also one line on each side of the diameter  $dm$ , equidistant from it, and parallel thereto. The distance of these two lines  $ao$ ,  $eq$ , from one another equal to the kerf of the saw, which divided the mirror. The diameter of this plate must be equal to that of the mirror before it was divided.

On the under side of the plate must be two pins fastened thereto,  $tt$ , their diameters equal to the kerf of the saw, that they may keep the two frustums of the mirror at the same distance from each other that they were before their division; so shall their circular edges be extended as far as the circumference of the plate, and their strait edges touch the said pins in the lines  $ao$ ,  $eq$ .

The end of the tube must be turn'd on the inside exactly to fit the plate and mirror, that they may not slide any way, for that would spoil the observations.

In the diameter of the plate  $rs$ , on the points  $v$ ,  $w$ , distant half an inch from  $c$ , the center of the plate,  
and

and a whole inch from each other, let a circle for the aperture of each frustum, of a proper size, according to the intended charge of the telescope, be described, and cut out. Also in the said diameter, equidistant from the center  $c$ , viz. at  $x$  and  $z$ , let there be a screw for each frustum, to elevate it a little from the plate, as shall be needful.

Let there be a spring contrived to press on the backside of the one frustum  $ora$  against the point  $v$ , being the middle between the edge  $ao$ , and the screw  $x$ , to keep the frustum close to the plate at the points  $ao$ , and also close to the screw  $x$ , when it is screw'd in. Let the like be also done on the back of the other frustum  $esq$ .

I say then,

1. That before the two screws are put in at  $x, z$ , the two frustums of the mirror will lie plain on the plate of brass, and have one pole at  $c$  common to them, and, consequently, will collect all rays, which, during their incidence, are parallel to the axis of the tube to one common focus in the said axis of the tube, just as they would have done before the mirror was divided.

2. But when the two screws  $x, z$  are put in their places, and screw'd a little way through the brass plate, they will lift the two frustums free from the plate at their circular edges, viz. at  $r$  and  $s$ , while their strait edges,  $ao, eq$ , are kept to touch the plate with both their ends (not in the middle, by reason of the mirror's concavity) by the pressure of the springs, as was mention'd above. By this means the pole  $c$  of the frustum  $ora$  will be removed from  $c$  toward

toward  $r$ ; and likewise the pole  $c$  of the other frustum  $esq$  be removed from  $c$  towards  $s$ , more or less according to the quantity of the elevation of each frustum by the screw that raiseth it: so that now there will appear at the focus two solar images; whereas there was but one, before the screws were put in.

By moving the screws, the two solar images may be brought to any distance from one another; but care must be taken not to raise one frustum more than the other, and the two solar images must almost touch one another at the time of the perigee; otherwise it must be better adjusted.

This telescope may be finished with a small elliptical specillum of black glass, ground plain on its reflecting surface, and a convex eye-lens, like that described by J. Hadley, Esq; F. R. S. in *Phil. Trans.* N<sup>o</sup> 376. A micrometer may be contrived for it at the common focus, near the eye-lens.

Such a double object-speculum would be capable of a vast improvement, by combining it with a concave specillum, which would reflect the images thro' a hole in the center  $c$  of the said speculum to fall on a convex eye-lens, after the manner of our new sort of reflecting telescopes, was it not for the difficulty of adapting such a micrometer to it as would exactly measure minutes and seconds; for the eye-glasses of such having usually a pretty large focal length, would bear much larger divisions on a micrometer, than Mr. Hadley's with a small eye-glass can do, tho' their charges should be equal, or that of the former did exceed.

I find that large object-glasses for telescopes are not commonly well center'd, with their poles in the very middle

middle of them. I sent for two to London, both of which were faulty; I therefore return'd them, and had two sent me again, as eccentric well nigh as the former ones.

Harris's *Lexicon Techn.* Vol. I. (see *Optics*) gives a rule for centering optic-glasses; but I think the following may be more sure and handy for a glass-grinder's use, and soon try whether a convex lens is well center'd.

Fig. 8.

Represents a round plate of brass, conveniently thick, and well harden'd by hammering (were it not for the rust, harden'd steel would be better), having many notches round it, one a little wider than that which is next to it, and number'd 1, 2, 3, &c. in their proper order, each of them wider at the bottom than at the entrance. I fitted such a notch to the thickest side of one of the glasses I had from London, so as the edge enter'd it but a little way, not half the depth thereof; but, on trying the opposite side, it went in, the whole depth thereof, and would have gone deeper, if the notch had been so cut: I then ground the lens narrower on that side which was thinnest, until I found it was at that place as thick as where I first try'd it in the notch. After this manner I reduc'd the glass to an equal thickness on its four quarters, and then ground off from other places what was needful to bring it circular. I also took care, when I tried it in the notch, that the lens should not be warmer on the one side than on the other by grinding, but tarried till I thought it thoroughly cold; and was also careful not to thrust it in harder on the one side than on the

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opposite

opposite side; for I could plainly observe a difference afterward, if I neglected to mind both these circumstances, or indeed either of them \*.

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XXVII. *A Description of a Contrivance for measuring small Angles, by Mr. John Dollond; communicated by Mr. J. Short, F. R. S.*

Read May 10, 1753. **L**ET an object-glass, of any convenient focal length (being truly ground and well center'd) be divided into two equal parts or segments, by cutting it straight through the center; and let a piece of machinery be so contriv'd, as to hold these two segments in the same position to each other, as they stood in before they were cut asunder; and to be capable at the same time of drawing them to different distances from that position, in the manner, as is represented in the figure.

Each of these segments will form a distinct image of any object, to which they are directed; differing in nothing from that, which might have been made by the whole glass before it was cut, except in brightness. And while these segments are held in their original position, the images will coincide, and become one single image as at first; but, in proportion as they

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\* Dr. Smith, in his *Complete System of Optics*, published in 1738, has described a very accurate and ready method of centering object-glasses, which was always used by the late Mr. George Graham, from whom the doctor had it.



they are drawn off from that situation, the images will separate more or less, according to the distance they are drawn to. By this means the images of two different objects, or of different parts of the same object, not very far from each other, may be brought to a contact or coincidence at the focus: and this coincidence may be view'd to a very great nicety with a proper eye-glass.

The measure of the angle subtended by the two objects, whose images are thus brought to a coincidence, depends upon three things: First, a careful observation of the coincidence of the images: Secondly, an exact measure of the distance, which the glasses are drawn out to from that situation, which makes the image single: And, lastly, a true knowledge of the focal distance of the glass. How the angle is to be found from these measures, and how it may likewise be come at, by viewing two land-objects at a convenient distance, will be shewn hereafter in the explanation of the figure. It is easy to understand, in the mean time, that the angle will be measur'd with more accuracy, in proportion to the length of the glass, which is used for that purpose; but the difficulty of managing long telescopes is no less apparent. Therefore the most practicable method of using this micrometer to advantage, is, to apply the divided object-glass to the object-end of a reflecting telescope: for, as the apertures of these sort of telescopes are large in proportion to their lengths, they will admit of very long glasses; nor will the measures be any way affected by the metals or glasses, which the reflector is composed of: and the angles will be found in the same manner, as though the images were view'd with a

single eye-glass, in the manner of a common refracting astronomical telescope; but with this advantage, that, as the images will be exhibited larger and distincter by the reflecting telescope; and as every part thereof will be much more manageable than a long refracting telescope; so the contact or coincidence of the images will be more accurately observ'd.

It would be however unnecessary now, as well as improper, to say much about the advantages of this method above those, which have hitherto been put in practice; because, as a machine is now making for this purpose, the experiments, which will shortly be tried, will be more convincing, as well as more intelligible, than any thing that might be offer'd at present.

### *Explanation of the Figure*

The two semicircles represent the two segments of the object-glass, whose centers *C* and *D* are drawn off to the distance *CD*, and the points *A* and *B* are two objects, or different parts of the same object; therefore the lines *ACG* and *B DG* represent two rays that pass thro' the centers or poles of the segments, and are therefore not at all refracted, but go straight through to *G*, where they intersect; and *G* being the respective focus to the distance of the objects from the glass, the two images will coincide at that point. It appears from the figure, that  $AB : CD :: GH : GE$ ; and from a common proportion in optics,  $GH : GE :: HE : EF$ . Therefore,  $AB : CD :: HE : EF$ ; *F* being the focus of parallel rays; and consequently

consequently the angles  $AE B$  and  $CF D$  are equal. That is, the angle subtended by the distance of the centers of the segments from the distance of the focus of parallel rays is equal to the angle subtended by the distance between the objects  $A$  and  $B$  from the end of the telescope.

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XXVIII. *A Letter to Sir Peter Thompson, Knt. F. R. S. containing Experiments on the Copper Springs in Wicklow in Ireland, and Observations thereon, by John Bond, M. D.*

S I R,

Read May 10,

1753.

**Y**OU may remember I had the Honour of spending an evening with you in June last, and happen'd to mention a spring in the county of Wicklow in Ireland, which was supposed to have the surprising quality of changing iron into copper. But your constant love of truth, and strong aversion to vulgar errors, made you doubt the fact, which, at that time, I could only affirm on the report, which prevailed among the curious in that country. You then propos'd several judicious queries, and seem'd desirous of being further inform'd concerning it; which rais'd my curiosity to take the first opportunity of inquiring more particularly into the foundation of that marvellous account.

Having soon afterwards occasion to go to Dublin, I went to the spring, which is from thence about thirty-eight miles, and made several experiments on the

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the water; the result of which I beg leave to present you with, hoping it may afford you some satisfaction in explaining that process, of which you so justly doubted the account given by some credulous authors, who mistook it for a real transmutation; a ridiculous doctrine, which destroys the essential qualities of bodies, which were impressed by the Great Creator on all material substances, in order to distinguish them from each other, and therefore are intransmutable.

As the history of this discovery has already been accurately related, in several papers read before the Royal Society, of which you have the honour of being a worthy member, I shall endeavour to avoid repetition, and confine myself to the chemical analysis of the water, which, I am told, has been hitherto omitted.

This water flows from a rich copper mine, and is of a sharp acid taste, and light blue colour. It is received and collected in pits, wherein iron bars are put, which, after lying in the water about three months, are intirely consumed, and at the bottom of the pits, a quantity of copper, greater than that of the iron is found, in the form of coarse sand. This fact is confirmed by profitable experiments, often repeated since the discovery, the honour of which is due to Mr. Matthew Johnston, a worthy old gentleman, and one of the proprietors of the mine, who first proposed this method of collecting the copper.

As this effect is certainly produced by some active principle in the water, I shall next inquire, what this principle is, and mention particularly the experiments, which I made with this view, that you may the better judge of the inferences deduced from them.

*Experiment 1.*

Into some of this water, taken out of the stream above the pits where the iron bars are placed, I pour'd a solution of an alkaline salt, which rais'd a strong effervescence, and precipitated a large quantity of a dark-brown substance.

*Corollary.* This experiment shew'd, that the water contain'd a strong acid, with a solution of the substance precipitated.

*Experiment 2.*

I put some aqua-fortis, or spirit of nitre, into some water taken out of the same place; and observed, that the strong acid immediately destroy'd the blue colour.

*Corollary.* From this experiment we may conclude, that the substance, which was precipitated by the alkali in the first experiment, was so perfectly dissolved by the acid spirit in the second, as to transmit all the rays of light.

*Experiment 3.*

Some small iron nails put into this water were in four minutes so closely cover'd with some substance of a copper colour, that, with a magnifier of half an inch focus, I could not discern the iron through it. In that time the nails gained four grains. The water had the same effect on silver and tin, but not on gold.

*Corollary.* The colour and increase of weight were owing to the adhesion of the particles of the matter dissolved in the water by an acid, that could not penetrate gold.

*Experiment*

*Experiment 4.*

In order to determine the quantity and quality of the matter in the water, I put two drachms of small iron nails into three ounces of it, and let them stand twenty-four hours; then examin'd, and found the surface of the water cover'd with a thick scum, like that of a chalybeat Spa. It lost the blue colour, and sharp vitriolic taste. It was quite transparent, and at the bottom there was a quantity of a brown ponderous powder, which, when dried, weigh'd 14 grains. This powder, melted without any flux, produced 12 grains of pure copper. The nails lost 8 grains in the water, and were, in several places, cover'd with a solid lamina of pure copper. The water, in which the nails lay, after being filtrated and evaporated, afforded a green vitriol, which in every respect resembled *sal Martis*, and produced the same effects, when dissolved, and mix'd with any astringent tincture.

*Experiment 5.*

From the spring-water treated in the same manner, I obtain'd a blue vitriol, the basis of which is copper.

From all these experiments it appears, that a mineral acid is the active quality in this water; which being diffused thro' the copper ore, unites itself with that metal, and forms a vitriol, which is dissolved by the water, and remains suspended in it, till it meets with iron in the pits, by which this acid is more strongly attracted than by the copper; therefore

fore it quits the copper, corrodes the iron, and changes it into a vitriol, which is again dissolv'd, and carried off in the stream continually flowing from the pits; whilst the copper, deserted by the acid, falls, by its specific gravity, to the bottom of the pits.

By this account it is evident, that this admirable process is a simple precipitation of the copper, by means of the iron. Hence it has been improperly called a transmutation of iron into copper. But, lest any difficulty should still remain, concerning the consumption of the large quantities of iron put into the pits, I shall add the following observations, to shew, that it is dissolved and carried off in the water.

*Observation 1.* The water in the pits is cover'd with a thick scum, occasion'd by the air bubbles constantly rising, and bursting on the surface; which is an evident sign of the solution of the iron.

*Obs. 2.* The iron is gradually consum'd in the pits, and abounds with irregular depressions, like old iron: a strong symptom of its being corroded by an acid.

*Obs. 3.* The chanel of the stream running from the pits is furr'd with red oker, which, after being roasted in a strong fire, was attracted by the magnet. As this oker is only found in the stream below the pits, it appears to be part of the iron dissolv'd in the water.

*Obs. 4.* The quantity of copper found in the pits after the iron disappears, is generally greater than that of the iron when first put in: for the proprietors assured me, that sometimes a ton of iron will produce or rather precipitate, a ton and half of copper.

This fact alone would be sufficient to prove, that the iron is not converted into copper; since, according to Sir Isaac Newton's table, the specific gravity of copper is to that of iron as 9000 to 7645.

When I was at this spring in August last, it ran at the rate of 12 ounces every second; and by putting iron into the water of the stream running from the pits, I found, that every ounce contained three grains of copper. Hence by calculation it appears, that 129600 grains of copper are carried off every minute, and consequently 124100 pounds Troy weight in a year; supposing the quantity and quality of the water to continue the same.

Hence we may easily account for the death of the fish, and other phænomena in the river, which receives this vitriolic stream.

In a hot sunny day, when the water is exhaled, the heaps of mold, raised out of the ore-pits, are cover'd with a vitriolic efflorescence: hence, in rainy weather, the water appears like a strong solution of verdegrise.

Whoever is desirous to imitate the process carried on in these pits, may readily gratify his curiosity, by putting pieces of iron into strong solutions of vitriol.

It is a common experiment, to tinge polish'd iron by rubbing it with Roman vitriol; which depends on the cause before-mention'd; viz. the acid in the vitriol penetrates the iron, and leaves the copper on the surface. This experiment is also taken notice of by that excellent chemist, and celebrated philosopher, Mr. Boyle, who calls it a *sympathetic precipitation*, in his *Essay on specific Medicines*.

As.



As soon as the attraction between the copper and the acid ceases, the mutual attraction between the minute particles of the metal prevails, so as to form large solid masses at the bottom of the pits, nineteen twentieths of which are pure copper. These solid masses are partly occasion'd, by the pressure of the incumbent heap of granulated copper, constantly increasing.

Hence we see, that the art of assaying, or separating metals from their ores, chiefly consists in evaporating an acid, which prevents the mutual attraction of the metallic particles: for when the acid is driven off by the violence of fire, the particles fall into their proper sphere of attraction, and assume a solid form.

From what has been offer'd on the theory of this admirable process, several practical hints may be taken to render such springs more profitable; and perhaps an easier method may be discover'd of separating copper from its ore, by precipitation, than by calcination. But such improvements I refer to those more conversant in the practice of metallurgy.

Having, I hope, given a satisfactory account of the effects of this water on iron, and proved, that it is owing to a strong mineral acid, which it contains, I should next inquire into the source of this acid; but am anticipated in that conjectural research by many inquisitive gentlemen, who have penetrated deeper into the bowels of the earth, and discover'd, or rather taken for granted, a vague acid, which, they say, is diffused thro' the whole mineral kingdom, and being united with different mineral substances, forms vitriols of different kinds. This hypothesis appears the more

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probable,

probable, since it is allow'd by modern chemists, that all acid spirits, tho' extracted from different minerals, produce the same effects, and consequently are of the same nature, the spirit of sea salt only excepted, which alone dissolves gold.

Since vast quantities of sulphur are found in this mine, mix'd with the copper-ore, it is probable, that the acid of the sulphur is attracted by the copper, and formed into a vitriol, which is afterwards dissolved by the water.

A farther inquiry into the origin of this acid, I leave to those curious gentlemen, who have solv'd the phænomena of volcano's, earthquakes, hot-springs, &c. from its effects.

The ingenious Dr. Robinson has lately illustrated this doctrine of acids \*, by proving, that an acid and light are the same thing: for he infers, from Sir Isaac Newton's philosophy, that whatever attracts, or is much attracted, is light: therefore an acid is light.

This spring perhaps is as remarkable for its medicinal as its metallic qualities. Tho' physicians generally reckon copper taken internally poisonous, yet the miners and other people drink this water frequently, without any ill consequences. It purges and vomits severely, and is become their specific in several diseases, particularly in cutaneous eruptions, arising either from an alkaline acrimony in the blood, which stimulates the sensible extremities of the cutaneous arteries, and occasions a pustule, or from the irritation of insects lodg'd in the skin; both which causes may be removed

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\* See his Essay on the Operations of Medicines.

removed by the strong acid in this water. It is an excellent detergent for scorbutic ulcers, as Hoffman justly observes. It has already perform'd several remarkable cures of this kind. I have often recommended it in such cases with success, join'd with proper internal medicines; for I am no friend to specifics.

How far the success of practice of the miners, who drink it frequently, may be depended on, longer experience must determine. Certainly, a great allowance must be made for the strength of their constitutions, and the insensibility of their nerves, constantly exposed to the noxious steams of damp pits. I never ventur'd to prescribe it internally; and as the *materia medica* affords vomits and purges of a more innocent kind, I think it in that respect unnecessary. I have reason to imagine, from the effects, which this water had on some earth-worms, that it is a very powerful anthelminthic, if cautiously given.

Some fresh filings of iron, put in this water, soon precipitate all the copper, and make it a strong and agreeable chalybeate. Hence it may be us'd as a substitute to Spa-water, whose virtue depends on the iron. Some prepared filings of iron remain'd eight days in this water; without producing the least alteration. Hence it appears, that this medicine can have but a weak effect, if any at all, in absorbing acids in the first passages.

Thus having communicated all, that at present occur'd to me, relating to this spring; if these observations render the history of it more perfect, and in the least contribute to gratify that laudable curiosity, which you always shew, in removing prejudices, rectifying

rectifying mistakes; and encouraging every branch of useful knowledge; I shall think my time well employ'd in this inquiry, which had its rise from your instructive conversation. I am, with the highest respect,

S I R,

London, April 15,  
1753.

Your most obedient, and  
most humble servant,

John Bond.

XXIX. *A Letter from Dr. Bevis to Mr. James Short, F. R. S. concerning Mr. Gascoigne's Invention of the Micrometer.*

Dear Sir,

Read May 17,  
1753.

**A**LTHOUGH Mr. Townley, in his letter to Dr. Croon, printed in the *Philosophical Transactions*, N<sup>o</sup> 25, p. 457, has sufficiently made appear, that the invention of the micrometer was Mr. Gascoigne's, and that he applied it to measuring small angles in the heavens, and for settling the moon's parallax, long before Messieurs Auzout and Picard thought of any such matters; yet are the French astronomers at every turn for giving it to these their countrymen, without so much as once mentioning the name of Mr. Gascoigne,

No sooner had the late Dr. Derham restor'd the application of telescopic sights to quadrants to its true author Mr. Gascoigne, than M. de la Hire, who never made the doctor any reply on that head, took occasion,

tion, in the memoirs of the Royal Academy of Sciences for 1717, to ascribe this contrivance of the micrometer to M. Auzout, in conjunction with M. Picard; alleging, for proof, an extract of a letter, dated Dec. 28, 1666, from M. Auzout to M. Oldenburg, and printed in *Phil. Trans.* N<sup>o</sup> 21. Several others have since copied M. de la Hire's assertion, and last of all, M. Bouguer, in the memoirs of the Royal Academy of Sciences for 1748, lately publish'd, wherein he describes an instrument, which he calls an heliometer; the contrivance whereof seems in every respect the same as that sent about ten years ago to the Royal Society, by Servington Savery, Esq;.

I have now before me the copy of a letter of Mr. Gascoigne to Mr. Oughtred, which I made myself from the original, written in 1640-1; which original was in the possession of the late William Jones, Esq; F. R. S. and is now in the library of the right honourable the Earl of Macclesfield. It consists of several sheets of paper, all about his invention for measuring small angles to seconds; where he not only gives the geometrical and optical principles of his contrivance, and the construction of the instrument, but also a series of observations actually taken therewith; some of which I shall transcribe.

1640 Aug. 5.	Jupiter's diameter	''	''	0 51
	Mars's	''	''	0 38
Dec. 24.	Mars's	''	''	0 25
	Venus's	''	''	0 25

1640 Aug. 25	Moon's semidiam.	15	17	h. 8 p. m.
Sept. 19,	''	''	''	15 11
				1640

1640 Oct. 9	;	.	.	16 36 h. 8 p. m.
10	.	.	.	16 36
27	.	.	.	15 38 h. 7 p. m.
29	.	.	.	15 41
30	.	.	.	15 43
31	;	.	.	15 49

These may suffice to prove, that Mr. Gascoigne's micrometer was not a mere thing in embryo, but brought to a good degree of perfection above 40 years before that of the French gentlemen was ever so much as mention'd. I am,

S I R,

Red-lion street, Clerkenwell, Your very humble servant,  
May 10, 1753.

J. Bevis.

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XXX. *Observations of the Transit of Mercury over the Sun, May 6, 1753; by Mr. J. Short, F. R. S.*

Read May 17, 1753. **T**HE instrument prepared for these observations was a reflecting telescope, of two feet focal length, of the Gregorian form, magnifying about 65 times, and so constructed in its machinery as to move in a plane parallel to the horizon, and also, when required, to move in a plane parallel to the equator.\* This telescope had two eye-pieces, each a combination of two glasses, viz. one eye-piece for the horizontal motion, with wires at right angles to one another, the wires being between the

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the glaffes, and one of the wires placed parallel to the horizon, and consequently the other was vertical; the other eye-piece was also a combination of two glaffes, and adapted to a micrometer, the glaffes being placed between the wires of the micrometer and the eye of the observer, and was to be used when the telescope moved in a plane parallel to the equator.

Mr. Short's house, in Surry-street, being so situated as not to see the sun at rising, the Rev. Dr. Birch, Secretary of the Royal Society, was pleased to allow us the use of his leads on his house in Norfolk-street, from whence we should be enabled to see the sun soon after he rose. We chose the doctor's leads, that we might not be far from Mr. Short's clock, which was easily within call, in order to compare a second-watch, which we made use of, with the clock, at every observation.

About half an hour after 4. o' clock in the morning of the 6th of May, Dr. Bevis and Mr. Short went to Dr. Birch's house, where the following observations were taken with the above reflecting telescope moving parallel to the horizon, and the eye-piece with the wires at right angles; Dr. Bevis observing, and Mr. Short writing down the times.

### By Mr. Short's clock.

h   '   "			I.
5	20	25	☉ upper limb at the horizontal wire.
	20	29	☉ preceding limb at the vertical wire.
	22	9½	☿ center at the vertical wire.
	22	33	☿ center at the horizontal wire.
	23	19	☉ subsequent limb at the vertical wire.
	24	0	☉ under limb at the horizontal wire.

# [ 194 ]

By Mr. Short's clock.

I. "

## II.

- 25 24    ⊙ upper limb at the horizontal wire.
- 25 27    ⊙ preceding limb at the vertical wire.
- 27  $4\frac{1}{2}$     ♀ center at the vertical fine.
- 27 34    ♀ center at the horizontal wire.
- 28  $17\frac{1}{2}$     ⊙ subsequent limb at the vertical wire.
- 29  $0\frac{1}{2}$     ⊙ lower limb at the horizontal wire.

## III.

- 31 45    ⊙ upper limb at the horizontal wire.
- 31  $55\frac{1}{2}$     ⊙ preceding limb at the vertical wire.
- 33 32    ♀ center at the vertical wire.
- 33  $52\frac{1}{2}$     ♀ center at the horizontal wire.
- 34 47    ⊙ subsequent limb at the preceding wire.
- 35  $20\frac{1}{2}$     ⊙ under limb at the horizontal wire.

## IV.

- 36  $44\frac{1}{2}$     ⊙ upper limb at the upper wire.
- 36 54    ⊙ preceding limb at the vertical wire.
- 38 29    ♀ center at the vertical wire.
- 38  $49\frac{1}{4}$     ♀ center at the horizontal wire.
- 39  $45\frac{1}{2}$     ⊙ subsequent limb at the vertical wire.
- 40 18    ⊙ under limb at the horizontal wire.

## V.

- 41 8    ⊙ upper limb at the horizontal wire.
- 41 25    ⊙ preceding limb at the vertical wire.
- 42  $57\frac{1}{2}$     ♀ center at the vertical wire.
- 43 13    ♀ center at the horizontal wire.
- 44 16    ⊙ subsequent limb at the vertical wire.
- 44 39    ⊙ under limb at the horizontal wire.

## VI.

- 47  $45\frac{1}{2}$     ⊙ upper limb at the horizontal wire.
- 47  $52\frac{1}{2}$     ⊙ preceding limb at the vertical wire.



By Mr. Short's clock.

h i ff

- 5 49 23 ♀ center at the vertical wire.  
 49 52 ♀ center at the horizontal wire.  
 50 53 $\frac{1}{2}$  ♂ subsequent limb at the vertical wire.  
 51 19 ♂ under limb at the horizontal wire.

## VII.

- 55 44 $\frac{3}{4}$  ♂ upper limb at the horizontal wire.  
 55 51 $\frac{1}{2}$  ♂ preceding limb at the vertical wire.  
 57 19 $\frac{3}{4}$  ♀ center at the vertical wire.  
 57 48 ♀ center at the horizontal wire.  
 58 44 ♂ subsequent limb at the vertical wire.  
 59 15 $\frac{1}{2}$  ♂ under limb at the horizontal wire.

## VIII.

- 6 1 29 ♂ upper limb at the horizontal wire.  
 1 37 $\frac{1}{4}$  ♂ preceding limb at the vertical wire.  
 3 4 ♀ center at the vertical wire.  
 3 32 $\frac{1}{2}$  ♀ center at the horizontal wire.  
 4 31 $\frac{1}{4}$  ♂ subsequent limb at the vertical wire.  
 4 58 ♂ under limb at the horizontal wire.

## IX.

- 5 31 $\frac{1}{2}$  ♂ upper limb at the horizontal wire.  
 5 39 $\frac{1}{2}$  ♂ preceding limb at the vertical wire.  
 7 5 ♀ center at the vertical wire.  
 7 33 $\frac{3}{4}$  ♀ center at the horizontal wire.  
 8 33 ♂ subsequent limb at the vertical wire.  
 9 1 $\frac{1}{2}$  ♂ under limb at the horizontal wire.

## X.

- 13 42 $\frac{1}{4}$  ♂ upper limb at the horizontal wire.  
 13 45 ♂ preceding limb at the vertical wire.

[ 196 ]

By Mr. Short's clock.

h ' "

- 6 15 8 ♀ center at the vertical wire.  
 15 41 ♀ center at the horizontal wire.  
 16 39 ⊙ subsequent limb at the vertical wire.  
 17  $8\frac{1}{2}$  ⊙ under limb at the horizontal wire.

## XI.

- 18 17 ⊙ upper limb at the horizontal wire.  
 18  $22\frac{1}{2}$  ⊙ preceding limb at the vertical wire.  
 19  $44\frac{1}{2}$  ♀ center at the vertical wire.  
 20  $17\frac{1}{2}$  ♀ center at the horizontal wire.  
 21 17 ⊙ subsequent limb at the vertical wire.  
 21  $44\frac{1}{2}$  ⊙ under limb at the horizontal wire.

About half an hour after 6, Dr. Bevis and Mr. Short went to Mr. Short's house, the sun then shining into his windows, in order to be near the clock, where the following observations were made in the same manner as before, Dr. Bevis observing, and Mr. Short writing down the times, in the presence of the right honourable James Earl of Morton, a fellow of this Society.

By Mr. Short's clock.

## XII.

h ' "

- 7 1 32 ⊙ upper limb at the horizontal wire.  
 1  $43\frac{1}{2}$  ⊙ preceding limb at the vertical wire.  
 2 50 ♀ center at the vertical wire.  
 3  $24\frac{1}{2}$  ♀ center at the horizontal wire.  
 4 39 ⊙ subsequent limb at the vertical wire.  
 4 57 ⊙ under limb at the horizontal wire.

## XIII.

By Mr. Short's clock.

XIII.

h ' "

- 7 6  $35\frac{1}{2}$  ☉ upper limb at the horizontal wire.  
 6  $41\frac{1}{2}$  ☉ preceding limb at the vertical wire.  
 7  $45\frac{1}{2}$  ♀ center at the vertical wire.  
 8  $28\frac{1}{2}$  ♀ center at the horizontal wire.  
 9  $35\frac{1}{2}$  ☉ subsequent limb at the vertical wire.  
 10  $0\frac{1}{2}$  ☉ subsequent limb at the horizontal wire.

The telescope, which hitherto moved parallel to the horizon, was now altered, to move parallel to the equator, and the eye-piece with the micrometer was applied, about half an hour after seven o' clock, Mr. Short observing, and Dr. Bevis writing down the times of observation.

h ' "

- 7 31 33 distance between the center of ♀ and ☉'s  
 under limb = 19 rev. 6 parts of the mi-  
 crometer.  
 36  $7\frac{1}{2}$  ☉'s preceding limb at the horary wire.  
 36  $56\frac{1}{2}$  ♀ center at the horary wire.  
 37 56 ♀ center distant from the ☉'s upper limb  
 = 34 rev. 14 parts.  
 41 33 ☉'s preceding limb at the horary wire.  
 42  $21\frac{1}{2}$  ♀ center at the horary wire.  
 43 21 ♀ center distant from the ☉ upper limb  
 = 34 rev. 23 parts.  
 44 34 ☉ preceding limb at the horary wire.  
 45  $21\frac{1}{2}$  ♀ center at the horary wire. At the same  
 time ♀ distance from ☉ upper limb =  
 34 rev. 29 parts.

2 1 11

- 8 4 25 ☉ preceding limb at the horary wire.  
 5 7 ☽ center at the horary wire. At the same  
 time ☽ distance from ☉ upper limb =  
 35 rev. 29 parts.  
 6 37 $\frac{1}{2}$  ☉ subsequent limb at the horary wire.  
 16 25 $\frac{3}{4}$  ☉ preceding limb at the horary wire.  
 17 6 ☽ center at the horary wire. At the same  
 time ☽ distance from ☉ upper limb =  
 36 rev. 19 parts.  
 18 39 ☉ subsequent limb at the horary wire.  
 44 1 $\frac{1}{2}$  ☉ preceding limb at the horary wire.  
 44 35 $\frac{1}{2}$  ☽ center at the horary wire. Presently  
 after ☽ distance from ☉ upper limb =  
 37 rev. 28 parts.  
 46 14 ☉ subsequent limb at the horary wire.

The sun's diameter perpendicular to the equator was taken in the micrometer of the reflecting telescope at 7<sup>h</sup> 25', and found to be = 53 rev. 13 parts, = 31' 50".

The sun's diameter perpendicular to the equator was taken in the micrometer of the 12 foot refracting telescope (which belonged to the late Mr. Graham) at about half an hour after 9 o'clock, and found to be equal to 51 rev. 14 parts, = 31' 31".

*Note,* The reflector's micrometer has 35 parts in one revolution of the screw; and the refractor's micrometer has 37 parts in one revolution of the screw.

Mr.

Mr. Short observed the last internal contact of mercury with the sun, with a four foot focal length reflector, magnifying about 135 times, at  $10^h 5' 7''$  by the clock, uncertain to 2 or  $3''$ ; and the total egress at  $10^h 7' 42''$  by the clock; uncertain to 5 or  $6''$ , the air then undulating thro' thin clouds.

Dr. Bevis observed the last internal contact with a two foot focal length reflector, magnifying about 65 times, at  $10^h 5'$ , and the total egress at  $10^h 7' 38''$ , by the clock.

Mr. Siffon, at Beaufort-Buildings in the Strand, observed the total egress at  $10^h 7' 43''$ , by Mr. Short's clock, thro' a five foot refracting telescope.

Mr. Bird, at his house in York-Buildings, observed the last internal contact at  $10^h 4' 57''$ ; and the total egress at  $10^h 7' 43''$ , by Mr. Short's clock, thro' a nine inch focal length reflector.

Mr. Smeaton in Furnival's-Inn-Court, Holborn, observed the total egress at  $10^h 8' 30''$ , by Mr. Short's clock, thro' a six foot refracting telescope. — He suspects his time some seconds too late, a cloud having just passed off the sun, when he perceived Mercury was gone.

Mr. Canton, in Spital-square, observed the total egress at  $10^h 8' 12''$ , mean time, thro' a reflecting telescope, three feet focal length.

Mr. Short's house in Surrey-street is  $26''$  of time west of the royal observatory at Greenwich.

*N. B.* Mr. Short's clock, by which these observations were made, was found to be  $28'$  slower than mean time: but, for more satisfaction, here are the sun's passages over the meridian, taken by the transitory.

May 7, Sun's preceding limb passed the	h	'	"
meridian at	-	-	II 54 38
Subsequent limb passed the mer.	II	56	51½
May 8, Sun's preceding limb passed the			
meridian, at	-	-	II 54 34½
Subsequent limb passed the me-			
ridian, at	-	-	II 56 47½

*N. B.* This clock, by repeated observations, was not found to have varied above 1" since the 22 of February last to the day of observation.

In the observatory of the right honourable the Earl of Macclesfield, at Shirburn-Castle in Oxfordshire, the total egress was observed at 10<sup>h</sup> 8' 11", apparent time. Latitude of Shirburn-Castle is 51° 39' 25", and its longitude is 4' 0" of time, west of Greenwich observatory.

XXXII. *An Account of a Treatise, presented to the Royal Society, intituled, " Letters concerning Electricity ; in which the latest Discoveries upon this Subject, and the Consequences which may be deduced from them, are examined ; by the Abbé Nollet, Member of the Royal Academy of Sciences of Paris, Fellow of the Royal Society, of the Institute of Bologna, &c." extracted and translated from the French, by Mr. William Watson, F. R. S.*

Read May 17, 1753. **T**HE work before us contains 262 pages in 12mo, exclusive of the preface, and four copper plates, representing seventeen figures.

This treatise is the production of a great master upon the subject of electricity : he has already published two volumes expressly thereupon, besides several memoirs among the works of the Royal Academy of Sciences at Paris. For several years he has done me the honour of being my correspondent, and I have communicated several valuable papers from him to the Royal Society.

The discoveries made in the summer of the year 1752 will make it memorable in the history of electricity. These have opened a new field to philosophers.

phers, and have given them room to hope, that what they have learned before in their museums, they may apply, with more propriety than they hitherto could have done, in illustrating the nature and effects of thunder; a phenomenon hitherto almost inaccessible to their inquiries.

But to make the most certain advantage of these new discoveries, we should confine ourselves to facts; and if we do draw consequences from them, they should be immediate and necessary ones; for, whenever our discoveries seem to promise to be useful and important, we are apt to hope and expect great success from them: we must therefore be careful to restrain our imagination, or we shall fall into error.

These considerations have induced our author to examine with care, what may truly be concluded from the experiments proposed by Mr. Franklin of Philadelphia, and since carried into execution in France, and elsewhere, in relation to the electricity of the clouds during a storm; by weighing every circumstance, and comparing the greatness of the effects, which have been had in view, with the more than apparent insufficiency of the means, which have been employed to produce them. He thinks, he sees clearly, that the considering the electrification of pointed bodies as a proof of lessening the matter of thunder, is abusing a real discovery to flatter ourselves with a vain hope; and it is chiefly to dissipate this error, if it yet subsists, that determined our author to print, in the work before us, some reflexions, which he had made at first only for himself, and a few persons, to whom he was desirous of communicating his opinion.

Mr.



Mr. Franklin's treatise upon electricity contains a great many very curious experiments; but the deductions from them being different from those, which the Abbé Nollet has given upon the same subjects, it might be imagined, if he were silent upon this head, that he had given up his former opinions. The honour, which the Royal Academy of Sciences has done them, in publishing them in their Memoirs, and the kind reception, which the public has given them, has obliged him to re-examine these opinions, and to undertake their defence; more especially as he sees, that he has powerful reasons to support them. This has also been a motive for the present publication, which our author is desirous should be considered, less as a criticism upon Mr. Franklin's doctrine, than as a defence of his own.

In some parts of these letters, our author mentions an electricity, which is very often, and perhaps always, in our atmosphere, when there is no appearance of thunder. He speaks of this, as if he only suspected it, and, in a manner, as if it wanted confirmation. He was then unacquainted with some decisive experiments made upon this subject by Dr. le Monnier\*, at St. Germain-en-laye, and which have been just published. He now considers, as a thing certain, that electricity is a very common meteor, which may manifest itself, when the weather is most serene;

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\* In a memoir read to the Royal Academy of Sciences at Paris, Nov. 15, 1752.

ferene; and that thunder is, strictly speaking, only one of its modifications, which renders it more sensible to us.

The Abbé Nollet's treatise contains nine letters; six of which are addressed to Mr. Franklin, one to Mademoiselle Ardinghelli, who, when only sixteen years old, translated Dr. Hales's treatise of Hæmatics into Italian, and added thereto some very ingenious remarks; one to Mr. Jallabert of Geneva, and one to Mr. Boze of Wittemberg: To these are added some experiments in electricity, made in support of opinions, laid down in this work, in the presence of Messieurs Bouguer, de Montigny, de Courtivron, d'Alembert, and le Roi, who were appointed by the Royal Academy of Sciences for that purpose.

In the first letter our author gives his correspondent Mademoiselle Ardinghelli an account of the discoveries in electricity in the year 1752; among which he takes particular notice of the experiment made on May 10, at Marly-la-ville, in consequence of Mr. Franklin's hypothesis; wherein pointed non-electrics, supported by electrics *per se*, gave manifest signs of electricity during a thunder-storm. This experiment, in the letters to Mr. Collinson, Mr. Franklin had proposed, but, as far as may be judged, had not then carried into execution. The experiment of Marly-la-ville was soon after verified by Dr. le Monnier at St. Germain-en-laye, who found further, first, that the like effects were produced, whether the iron rods were pointed, or not; and that it was indifferent, whether their position was horizontal, or not. Secondly, that thunder electrified not only iron, but also wood,

wood, living bodies, and other electrifiable substances. Thirdly, that it was not absolutely necessary to place these bodies at the tops of buildings; and that it was sufficient for them to be placed about four feet from the ground in an open situation, and at some distance from large buildings. Fourthly, that bodies electrified in this manner produced the like phenomena with those electrified by glass after the usual manner. It was afterwards discovered, that electrifiable bodies, thus disposed in open air, were sometimes electrified under thick clouds, but without thunder, lightning, or even without rain or hail.

The Abbé Nollet recommends, that these experiments should be made with circumspection, as he has been informed by letters from Florence and Bologna, that those, who have made them there, have had their curiosity more than satisfied by the violent shocks, which they have sustained, in drawing off the sparks from an iron bar electrified by thunder. One of these in particular says, that once, as he was endeavouring to fasten a small chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building, in order to draw off the electrical sparks by means of the oscillations of this ball, there came a flash of lightning, which he did not see, but which affected the chain with a noise like wild-fire. At that instant, the electricity communicated itself to the chain of the copper-ball, and gave the observer so violent a commotion, that the ball fell out of his hands, and he was struck backwards four or five paces. He never had been so much shocked by the experiment of Leyden.

From

From the experiment at Marly-la-ville, and those which have been made since, have been drawn two consequences ; one, that the matter of thunder, and that of electricity, are one and the same : the other, that, by the means of pointed iron rods, one might, without its doing any harm, draw off all the fulminating matter from a stormy cloud. But our author has shewn, that bodies being pointed are not absolutely necessary ; and is desirous, we should not too hastily believe, that mischiefs arising from thunder may be averted by the apparatus proposed. He thinks the means vastly too small for the greatness of the cause.

Our author's first letter to Mr. Franklin is an introduction to the five subsequent ones.

The second letter treats of the nature of the electric matter. In this its analogy with fire is considered and proved ; and our author takes notice, that Mr. Franklin, he imagines, who has certainly made some important discoveries into the properties of electricity, cannot but be dissatisfied with the editors of his work, for publishing, “ that he exhibited to “ our consideration an invisible subtil matter, disseminated throughout all nature, &c. which had hitherto escaped our observations.” The latter part of which assertion is not strictly true ; as the considering the matter of fire, and that of electricity, to be one and the same, is a fundamental principle of what both the Abbé Nollet and myself formerly published upon this subject.

The third letter to Mr. Franklin contains several proofs, that glass is not impermeable to the electric matter.

matter. Some of the experiments of our author upon this subject I heretofore did myself the honour to lay before you ; and they are in my opinion fully conclusive.

The fourth letter to Mr. Franklin relates to several phenomena of the experiment of Leyden. In this letter it is examined, whether the effects of this experiment proceed from the glass phial, or from the non-electrics contained therein ; and experiments are produced to prove, that the power of giving a shock in an electrified phial of water, proceeds from the water in the phial, and not from the phial itself, as Mr. Franklin imagines. In this letter likewise is an examination of Mr. Franklin's opinion, that, in the charged phial, as much fire as is received by one of its surfaces is lost by the other.

The fifth letter to Mr. Franklin is in relation to the power of pointed non-electric bodies drawing off and throwing off electrical fire, at a much greater distance than obtuse bodies do of the same kind. Our author thinks, that Mr. Franklin has attributed more power to pointed bodies, than, upon experiment he finds to be true.

The sixth letter to Mr. Franklin is upon the analogy of thunder with electricity. This is a fact at present so well established, as to admit of no doubt. But our author cannot agree with Mr. Franklin in his opinion, " that thunder is at present in the power of men, and that we are able to dissipate it at our pleasure : that an iron rod (such a one as Mr. Franklin has directed, and such a one as has been made use of) is sufficient to discharge of all its fire  
" a

“ a stormy cloud against which it is directed.” For his part, he confesses, that he cannot believe it ; first, because he sees too great a disproportion between the effect and the cause : secondly, because the principle, which is given us to support this opinion, is not sufficiently established. He can hardly think, that the fulminating matter, contained in a cloud, capable of covering a great city, can be drawn off in a few minutes by a pointed bar, as thick as your finger. If even a number of these placed upon the tops of eminencies were only necessary to prevent the effects of thunder, would not the vanes and crosses at the tops of our steeples have been sufficient to procure us this advantage ? These buildings however, in all times, have not been exempted from the mischiefs of thunder. He despairs of our weak efforts ever being able to disarm the heavens.

Our author here gives us the representation and description of his apparatus for electrifying during the thunder : it differs in nothing essential from those, which we used last year.

In this letter are likewise consider'd the validity of Mr. Franklin's hypothesis of electric and non-electric clouds ; the former arising from the sea, the latter from the land ; their operation, upon their approaching one near the other ; the difference, according to Mr. Franklin, between electrical and common fire ; and several other parts of Mr. Franklin's doctrine.

The eighth letter is addressed to our worthy brother Professor Jallabert of Geneva ; and, among other curious particulars, inserts part of a letter, which our author had received from Mr. Jallabert, giving an  
account

account of an experiment, which Mr. Jallabert had some time since made at the water-works at Geneva. An account of this experiment was communicated by myself to the Society; and it has near relation to the experiment, which we made here in electrifying the river Thames six years ago. Mr. Jallabert consults the Abbé Nollet in relation to the solution of the phænomena of this experiment; and the Abbé does me the honour now to give the same solution thereto, which I first gave to a similar experiment of Dr. le Monnier's, and laid before the Society in January \* 1746, and since applied upon other occasions in illustrating the electrical circuit.

The ninth letter is addressed to Mr. Bosc, professor of mathematics and philosophy at Wittemberg; and is in answer to one of Mr. Bosc, in which this gentleman expresses himself surprised, that so many ages have passed, without it having been discovered, that thunder electrifies bodies; since it depends upon an experiment so simple, and which it is hardly possible to fail in, when you desire to repeat it under proper circumstances.

Upon this our author observes, that people in general only see the facts, and are ignorant of, or do not consider, the means, by which philosophers arrive at them; nor perceive the circumstances, without which these phænomena could never have been made known to us; and that Mr. Bosc will cease to be surprised, as he is so well versed in these phænomena, when he reflects upon what our author offers.

To

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\* See *Phil. Trans.* Vol. XLIV. p. 388.

To make the experiment in question, it is necessary that bodies should be supported by glass, silk, or resin, without touching any thing else communicating with what we now call non-electrics; without which, the signs of electricity, which are sought for, cannot manifest themselves.

To this experiment, therefore, a previous knowledge is required, of insulating bodies to be electrified; but where is the man who was acquainted with this fact thirty years ago? Before that period, it was not even guessed at by any one.

Since Mr. Gray discovered, that bodies must be insulated, to communicate to them a perceptible electric virtue, to what purpose could we set up iron bars under a stormy cloud? This thought could not have happened, but to those who had taken notice of the analogy between lightning and electricity, and upon whom this idea had made a strong impression. And no one could think seriously upon this analogy, but since the discovery of the experiment of Leyden, that is, since the year 1746. Before that time the electrification of bodies by thunder could not have been perceived, but by an accident very difficult to meet, on account of the conditions requisite.

Nevertheless it may be urged, that bodies, being really electrified, have shewn themselves in all ages \*,

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\* I formerly took notice, that the electrical attraction had been observed so early, as to be mentioned by Theophrastus (see *Phil. Trans.* Vol. XLIV. p. 732); so its luminous appearance, though only considered as a meteor, is mentioned by Plutarch, in the life of Lyfander. Pliny, in the second book of his natural history, chap.



as historians both ancient and modern have made formal mention thereof. But to this it may be replied, that it was not enough to know the fact, unless people were enough acquainted with it to take it for what it really was; that is, the electric virtue: for without that, observations of this kind could have very little weight with

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37, calls these appearances *fars*; and tells us, not only that they settled upon the masts, and other parts of ships, but also upon mens heads: ‘*Exsistunt (says that historian) stellæ et in mari terrisque. Vidi nocturnis militum vigiliis inhærere pilis pro vallo fulgorem effigie ea: et antennis navigantium, aliisque navium partibus, ceu vocali quodam sono insistunt, ut volucres sedem ex sede mutant: . . . . geminæ autem salutare, et prosperi cursus prænunciæ; quarum adventu, fugari diram illam ac minacem, appellatamque Helenam, ferunt. Et ob id Polluci et Castori id numen assignant, eosque in mari deos invocant. Hominum quoque capiti vespertinis horis, magno præfagio circumfulgent.*” But (adds he) all these things are ‘*incerta ratione, et in naturæ majestate abdita.*’

Seneca, in his *Natural Questions*, chap. 1. takes notice of the same phænomenon. ‘*Gylippo (says he) Syracusas petenti visa est stella super ipsam lancem constitisse. In Romanorum castris visa sunt ardere pila, ignibus scilicet in illa delapsis.*’

*Cæsar de Bello Africano*, cap. 6. edit. *Amstel.* 1686. We here find them attending a very violent storm. ‘*Per id tempus fere Cæsaris exercitui res accidit incredibilis auditu; nempe Virgilii arum signo confecto, circiter vigilia secunda noctis, nimbus cum faxea grandine subito est exortus ingens. . . . Eadem nocte V legionis pilorum cacumina sua sponte arserunt.*’

Livy, chap. 22. mentions two similar facts: ‘*In Sicilia militibus aliquot spicula, in Sardinia in muro circumeunti vigilas equiti, scipionem, quem in manu tenuerat, arsisse, et litora crebris ignibus fulsisse.*’

These appearances are called by both French and Spaniards inhabiting the coasts of the mediterranean, *St. Helme* or *St. Telme*:

with any person engaged in the inquiry. At present, indeed, when we know, from the experiment of Marly-la-ville, that a stormy cloud is a great electric mass, the action of which extends itself sensibly even to bodies, which are upon the surface of the earth, we must agree, by reflecting on them, that the lights, which have been seen upon the crosses placed on the tops of several steeples, those, which the Roman soldiers said they had observed at the end of their pikes, and those lambent flames, which appear upon the masts of ships, which mariners call St. Helmo's fire, are so many electrical phænomena. But until the moment that this experiment was made, which open'd our eyes with regard to the possibility and nature of these marvellous effects, these appearances were regarded either as popular illusions, or false prodigies, or even as luminous vapours, which might be ranged in the class of *phosphori*. Moreover, as these were seen but seldom, if ever we had been tempted to attribute them to the influence of stormy clouds, we might have been dissuaded therefrom, by considering the little agreement there is, between the rarity  
of

fires ; by the Italians, the fires of St. Peter and St. Nicholas, and are frequently taken notice of by the writers of voyages.

If some late accounts from France are to be depended upon, we are informed, that at Plauzet it has been observed for time immemorial ; and M. Binon, the curé of the place, bears his testimony of the truth, that, for twenty-seven years, which he has resided there in that capacity, in great storms, accompanied with black clouds, and frequent lightnings, the three pointed extremities of the cross of the steeple of that place appear surrounded with a body of flame ; and that, when this phænomenon has been seen, the storm was no longer to be dreaded, and calm weather returned soon after.

of these effects, and the frequency of the causes, which might produce them.

We see, therefore, how important it is to describe exactly the phenomena we observe: otherwise, how long may it be, before we can deduce any real instruction from those, which we have been informed of in a negligent and superficial manner? We have heard all our lives of St. Helmo's fire, of those which the antients call Castor and Pollux, and of the comazants of our mariners. But, from what we have had related to us, and from what we have read, who could have been prevailed upon to range them with electrical phenomena? We have heard them represented, as thin lambent shining lights, a kind of phosphoreal vapour: but there is a passage in the memoirs of the Count de Forbin, quoted by our author, wherein mention is made of St Helmo's fire; which, if any one, well versed in the phenomena of electricity, had carefully attended to and considered a few years ago, he might have prognosticated success to Mr. Franklin, when he proposed his experiment upon thunder. " In the night (says the author of those memoirs) on a sudden it became exceedingly dark, and thunder'd and lightened most dreadfully. As we were threatened with the ship's being torn to pieces, I ordered the sails to be taken in: We saw, upon different parts of the ship, above thirty St. Helmo's fires: Among the rest, there was one upon the top of the vane of the main-mast, which was more than a foot and half in height. I ordered one of the sailors to take it down: When this man was on the top, he heard this fire; its noise resembled that of fired wet gunpowder: I  
" ordered

“ ordered him to lower the vane, and come down;  
 “ but scarce had he taken it from its place, but  
 “ the fire left it, and fixed itself upon the top of  
 “ the mainmast, from which it was impossible to  
 “ remove it; and continued there a considerable time,  
 “ until it went out by little and little, &c.”

If all the authors, who have taken notice of St. Helmo's fire, had spoken of it as this just quoted, philosophers might have reproached themselves for its having been so long before they had a just idea thereof, and for their not having shewn the principle upon which it depended. But how few historians are there, who could have related this fact with circumstances so proper to put us in a right train, as those just mentioned?

“ And here I cannot but observe, as I am con-  
 “ vinced, that the matter of thunder and that of  
 “ of electricity are one and the same, how vast an  
 “ idea must the attending to the before-mentioned  
 “ passage excite in the mind of persons, accustomed  
 “ to the phænomena of electricity? How immense  
 “ a quantity of it must they conceive to have been at  
 “ that time in the atmosphere surrounding the ship,  
 “ and within the verge of its action, to furnish more  
 “ than thirty St. Helmo's fires; the same, in fact,  
 “ which we see at the ends of our conductors in  
 “ electrifying, one of which was more than a foot and  
 “ half in height? At this time, and under these cir-  
 “ cumstances, the masts, yards, and every part of  
 “ the ship, I consider as conductors of electricity,  
 “ between the, at this time electrified, atmosphere,  
 “ and the sea: and tho', being of a vegetable nature,  
 “ and, if dry, even of the worst kind for this pur-  
 “ pose,

“ pose, they conducted electricity much less perfectly  
 “ than metal under the like circumstances, would  
 “ have done, I doubt not, but that they were greatly  
 “ instrumental in averting the danger, with which  
 “ the ship was threatened.

“ Upon these considerations, I do not scruple to  
 “ recommend, as Mr. Franklin has done, communi-  
 “ cations of metal between the spindles and iron-  
 “ work at the tops of the masts of ships, and the sea;  
 “ or, which will answer the same purpose, the bilge  
 “ water in the well. This can be liable to little  
 “ objection, as the doing it is neither difficult, nor  
 “ expensive; an iron wire, of the thickness of a goose-  
 “ quill, conducting electricity more readily than any  
 “ piece of timber, however large; and these masts  
 “ do it so much the worse, as they are of a resinous  
 “ nature.

“ From attending to these phenomena, we every  
 “ day see more and more the perfect analogy (to  
 “ compare great things with small) between the  
 “ highly electrified glass jar, in the experiment of  
 “ Leyden, and a cloud replete with the matter of  
 “ thunder. But more of this possibly upon some  
 “ future occasion.

“ Though the number and continuance of the St.  
 “ Helmo's fires, in the passage before-mentioned,  
 “ probably tended greatly to preserve the ship from  
 “ the destruction, with which it was then threatened,  
 “ yet the cause may be too great, and come on too  
 “ fast, to be lessened enough by these means to avert  
 “ the mischief. Thus in the account, published in  
 “ the \* *Philosophical Transactions*, from Captain John  
 “ Waddel,

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\* Vol. XLVI. p. III.

“ Waddel, his ship was almost beaten to pieces by  
 “ the thunder and lightning: although, as he expresses  
 “ himself, there were sundry large comazants over  
 “ head, some of which settled on the spindles on  
 “ the topmast-heads, and burnt like very large  
 “ torches. When this account was written, these  
 “ phænomena were only considered as the presages  
 “ or attendants of a storm, and no sort of inference  
 “ proposed from them.”

But to return to our author: His work closes with a series of experiments, intended to demonstrate the validity of the conclusions exhibited therein. These merit the particular attention of those conversant in these matters; but I must refer you here to the work itself, and only observe, that some of the experiments are made *in vacuo*, and are of the same kind with those which I communicated to the Royal Society in February 1752; and which have been since published in the *Philosophical Transactions* \*.

Upon the whole, I think the treatise before us a very valuable one, as it gives us the still riper thoughts of an able writer upon a difficult, and, till very lately, an almost unknown, subject; of one, who, besides his inquiries into this part of philosophy, has a great compass in the knowledge of nature, and is therefore well qualified to investigate her phænomena.

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\* Vol. XLVII. p. 363, et seq.

XXXII. *The Number of People in the City of Bristol, calculated from the Burials for Ten Years successive, and also from the Number of Houses; by John Browning, Esq; of Barton-hill near Bristol: Communicated by Henry Baker, F.R. S.*

Read Jan. 25, 1753.

The certificates were obtained under the hands of the præcentor of the college, the several ministers of the seventeen parish-churches, the register-keeper of the several Quakers cemeteries, the several Anabaptists cemeteries, the Jews new-erected cemetery, for ten years, including the year 1741 and 1750. Whereas some of the parishes within the liberties of the city do extend themselves beyond the liberties into the counties of Gloucester and Somerset, they are distinguished by the names of the out-parishes. The inhabitants of the several out-parishes being buried within the liberties of the city, must of consequence be brought into the city bill of mortality.

E c

Names

Names of the Parishes.	Burials.
The Cathedral . . . . .	23
All Saints . . . . .	58
St. Michael's . . . . .	637
St. Werburgh's . . . . .	101
St. James's . . . . .	2945
St. Mary Redcliff's . . . . .	1538
St. Thomas's . . . . .	639
Temple . . . . .	1398
St. Nicolas's . . . . .	844
St. Mary Port's . . . . .	245
St. Peter's . . . . .	638
St. Philip and Jacob . . . . .	3661
St. Ewin's . . . . .	30
St. John Baptist's . . . . .	538
St. Leonard's . . . . .	74
St. Stephen's . . . . .	1234
St. Augustin's . . . . .	916
Christ-church . . . . .	311
The Quakers Cemeteries . . . . .	506
The Baptists Cemeteries . . . . .	979
The Jews new-erected Cemetery . . . . .	2
Total Number Burials for Ten Years	<u>17317</u>

$17317 \div 10 = 1731$  Burials in each Year.

Rated



Rated Houses in each Parish.	Number.
All Saints . . . . .	42
St. Michael's . . . . .	375
St. Werburgh's . . . . .	43
St. James's . . . . .	1020
St. Mary Redcliff's . . . . .	420
St. Thomas's . . . . .	209
Temple . . . . .	211
St. Nicolas . . . . .	411
St. Mary Port . . . . .	106
St. Peter's . . . . .	181
St. Philip and Jacob (large out-parish)	363
St. Ewin's . . . . .	25
St. John Baptist's . . . . .	144
St. Leonard's . . . . .	54
St. Stephen's . . . . .	375
St. Augustin's . . . . .	480
Christ-church . . . . .	160
The Castle Precinct's Ward . . . . .	247
<hr/>	
Total number of rated houses	4866
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The latest and most accurate observations demonstrate, that in great cities a twenty-fifth part of the people die yearly.

The yearly amount of the burials at a medium for ten years,  $1731 \times 25 = 43275$ , the number of inhabitants.

The number of houses rated to the land-tax, as appears by the rates in the council- house, Michaelmas, 1751,	}	4866
As the rates are not always accurately made, and as it is the usual custom not to rate houses, which are untenanted, hospitals, or alms-houses, it will be necessary to make a very large allowance for these deficiencies, especially as many houses are rated in gross under the denomination of several tene- ments; when they belong to the same landlord in all the several parishes, an al- lowance of 25 <i>per cent.</i> will be more than sufficient	}	1216
The number of houses computed in the se- veral out-parishes	}	1200
Total number of houses		<u>7282</u>

The usual number of souls allowed to each house  
is six.

Houses

$7282 \times 6 = 43692$  number of inhabit<sup>s</sup>. by the houses.  
43275 number of inhabit<sup>s</sup>. by the burials.

XXXIII. *An Account of the Eclipse predicted by Thales; by the Rev. William Stukeley, M. D.*

Read May 3, 1753. **W**HILST I lived in Lincolnshire, I was visited by Mr. Edmund Weaver, who was a very uncommon genius; particularly he had made himself a great master of astronomy, and was scarcely to be accounted the second in the kingdom. He composed complete tables of the celestial motions, which he was very much solicited to publish; but the world waited for Dr. Halley's.

These tables were shewn to Dr. Halley: we may have a notion of their value from what the doctor said thereupon, that he suspected, Mr. Weaver had seen his tables. He was well known to, and much esteemed, by Mr. Martin Folkes. He taught himself writing, arithmetic, algebra, some sublime parts of the mathematics, the whole art and science of astronomy; as his annual publications sufficiently evince. He was an instance of great merit in obscurity: he died in a little house of his own, soon after I removed to London, Dec. 27, 1748. and was buried at Cathorpe near Grantham.

Thus much I thought proper to commemorate of this worthy person. An intimacy grew up between him and myself during that twenty years I lived in the country, nor was it unfruitful; for we often agreeably entertained ourselves in calculations of astronomy, with a view to antient history. One of them I here produce before the Society, done many years

years ago ; but, upon hearing that read on the same subject from Mr. Costard, it put me in mind of it, and I hope it will be an acceptable illustration and confirmation of that famous piece of history, the eclipse predicted by Thales the Milesian; which happened in the 603 year before the Christian æra.

I shall recite the history of this matter, as concisely as I can, from the historians and writers, tho' they all mistake the year. But this shews the admirable use to be made of astronomy in ascertaining matters of history.

The great king of Babylon, Nebuchadnezzar, was now busy in executing the vengeance, which God had made him the instrument of, upon the nation of the Jews, for their incorrigible wickedness and folly. Their king Jehoiachin was carried away captive to Babylon, and kept in prison 37 years together, till he died.

At this time there was a sharp war between the Medes and Lydians, of which Herodotus give us an account. Halyattes, father of the famous Cræsus, was now king of the Lydians.

After the Medes had conquer'd all the upper or northern part of Asia, from the old possessors the Scythians, they again extended their borders to the river Halys in Lesser Asia, the boundary between Cappadocia and Armenia, or between the Lydians and Medes. It was not long before there happened a war between these nations, which continued for five years together, with various success.

In the sixth year they engaged each other, with the utmost of their strength ; intending to make that battle decisive of the quarrel, that was between them:

but, in the midst of the engagement, whilst the fortune of the day seem'd to hang in an equal balance, there happened a total eclipse of the sun, which overspread both armies with a horrible darkness; inso-much that, being affrighted at such a critical judgment of heaven (as they thought it), both sides put up their swords; and they agreed to refer the controversy between them to two arbitrators. Halyattes, king of Lydia, chose Siennesis, king of Cilicia; Cyaxares, the Median monarch, chose Nebuchadnezzar, now busy in leading the Jews into captivity.

Nebuchadnezzar is by Herodotus called Libynetus. It seems to me, that the letter N, in the beginning of the word, has, in the antient copies of Herodotus, been turned into Λ; and then the words, in two different dialects, are not very different.

These great arbitrators compromised the matter between the contending parties, by making a match between the two royal families; and so restor'd peace and friendship. Astyages, the son of Cyaxares, king of Media, married Ariana, daughter of Halyattes, king of Lydia, of whom, a year after, was born Cyaxares, whom the prophet Daniel calls Darius the Mede. And in that last mention'd year, king Cyaxares gave his daughter Mandane in marriage to Cambyses king of Persia; of whom, the next year, was born the great Cyrus, the founder of the Persian monarchy, whom the prophet Isaiah foretold by name, that he should restore the polity of the Jews, the city of Jerusalem, and the temple, and return the sacred vessels of gold and silver, which Nebuchadnezzar had carried away, and put into his heathen temple at Babylon.

Thus

Thus ended this famous quarrel between the Medes and Lydians, thro' the timely event of a total solar eclipse, made still the more eminent, that it was calculated, and foretold to the Ionians by Thales of Miletus, at that time in the 37 year of his age. He was born of Phœnician parents; and there, no doubt, learned his knowledge in astronomy. He was the first, who brought this science into Greece, and that 300 years after the pretended Chiron of the Argonauts.

It is an invincible argument, that he learned his art; for a whole life is not sufficient, so to observe the motions of sun and moon, as to be able to calculate an eclipse.

This is the first eclipse, which we have recorded in so circumstantial a manner. Notwithstanding all this, it is strange to see, how the learned have erred about the true year of this memorable affair.

Pliny begins the mistake, telling us, that it was the fourth year of the XLVIII Olympiad; whereas it was the fourth year of the XLIII. It is not unlikely, the numeral letter V is crept into the original. Clemens Alexandrinus makes it about the fiftieth Olympiad. Dr. Prideaux makes it 5 years too late; Archbishop Usher 2 years. Sir Isaac Newton gives us the true month and day, but assigns the 585 year, as Ricciolus.

I have designed the map here exhibited, from my friend Weaver's calculation (Fig. IX.), which will present us with a just notion of the whole affair. It is a projection of the moon's shade, as it passed over the earth's surface from 20 to 60 degrees of longitude east from London; and from 25 to 50 degrees of  
north

north latitude, with the hours, half-hours, and quarters of time, where vertical. This was on the 18 of May in the proleptic Julian style: in the year of the Julian period 4111, the 603 year before the vulgar æra of Christ.

The eclipse was total 4 minutes and a half, where the battle was fought. The shade enters our map, in the desert of Barca in Africa, soon after 9 o' clock in the morning. It traverses the mediterranean sea, and isle of Cyprus; enters Asia Minor at Cilicia, a little before 11; about half an hour after, it passes the city now called Erzerum; near which I suppose the battle was fought, as being at the boundary between the two kingdoms. It is between the river Halys, and the river Melas, on which was the antient city Melitene. The river Melas runs eastward into the Euphrates. At half an hour after 12, the shade enter'd upon the Caspian sea, and at 1 upon the Kal-muc Tartary.

We see here an authentic *parapegma* in antient history, deduced from astronomy: and we see a remarkable instance, brought about by Divine Providence, of a most furious war, terminated by the intervention of an eclipse. But eclipses, say we, are natural and necessary phænomena, consequent to the established motions of the celestial bodies. True Providence order'd them at the beginning, as well as comets, and earthquakes, and the like portentous appearances, as antiquity rightly denominated them and regarded them: but Providence did not there by restrain its own power and authority, to render them at the same time of moral use. Providence can so over-rule the actions of us men, as to bring them to coincide with these fore-ordain'd and neces-

sary motions, so as to prove himself the GOVERNOR both of the natural and moral world ; tho' improv'd philosophy has given us a juster notion of these matters than the antients had. I wish our religious sentiments may advance, in proportion to our improved philosophy.

March 16, 1753.

Wm. Stukeley.

XXXIV. *A farther Account of the Giant's Causeway in the County of Antrim in Ireland, by the Rev Richard Pocock, LL.D. Archdeacon of Dublin, and F. R. S.*

Read May 24, 1753. **I**N a letter, which I wrote in 1747 to Martin Folkes, Esq; President of the Royal Society, which was read in January, and printed in the *Philosophical Transactions* for that month, I observed, in relation to the Giant's Causeway, that there appeared in the Sea-cliffs three strata of pillars between thirty and forty feet high, with strata of a black rock between them; that the causeway itself was the lowest of all these, extending in a point into the sea ; and that another is seen towards the top of the cliff.

Last summer I took another view of it; I went from Bally-Castle, which is about 10 miles to the east of the Causeway. When I came two miles to the west of Bally-Castle, within less than a mile of Ballintoy, half a mile to the south of the sea-cliffs, and about a quarter to the south of the road, I saw the  
same



same kind of pillars in a low hill ; I observed both hexagons and pentagons.

The rocks towards the sea appeared as if they were formed in the same manner ; but when I came to them, I found it was only common rock in several strata, and perpendicular joints.

I went on about two miles to a peninsula called Donseverik, where I saw some tendency in the rock towards this work of nature ; and going about half a mile farther, came to the beginning of the pillars in the sea-cliff, as I believe, about five miles from the causeway : and the shore and cliffs being shap'd mostly in little semicircular bays, I had many very beautiful views of the upper and middle strata of pillars : in one, particularly, they had much of the appearance of ruin'd portico's one over the other ; and turning the little end of a spy-glass, it appeared something like the ruins of Palmyra, as a view of them is represented in a copper plate, published in the *Philosophical Transactions*.

This wonderful work of nature is continued on in the cliffs for about a quarter of a mile beyond the Giant's Causeway.

I saw it again in the road to Coleraine, five miles to the west of the Causeway, in a low hill a furlong to the south of the road, and two miles to the south of the sea. The pillars here are small ; and being about a mile and a half from Ballinagarra, where the earl of Antrim has a ruin'd house, lately burnt down, it serv'd, as I suppose, as a quarry for building part of that house, in which I saw a great number of the stones, and particularly one of nine sides. I saw others near two miles farther, to the south of

the road in a low hill, within two miles of Coleraine; so that the whole extends for about eleven Irish miles, or fourteen English.

Beyond Coleraine, to the east of Magilligan, I saw in the rocks towards the sea-cliffs, the stones in the hills very regular, appearing at a distance much like these pillars. This is six computed miles beyond Coleraine, and consequently about ten English miles from the last pillars.

At Fairhead also, a high point of land, three miles to the east of Ballycastle; towards the top of it, the rock appears as in grand pillars. They say it is not in joints, but it has something of the appearance of a grand Gothic piece of workmanship.

As I spent a week at the Causeway, and sent away by sea to Dublin as great a variety of the stones as I could conveniently get, particularly a large octagon, with the eight large stones round it; a pair of less, with eight pair, that encompass it; two small pentagon pillars, about fourteen inches over, one of them three feet ten inches and a half high, the other five feet seven inches; one hexagon pillar, about the same size, and five feet five inches high; all which I have placed in my garden; so I have had an opportunity of considering it at leisure.

It is a black stone, weighty and brittle: and I have been informed, that it was tried in a glass-house, and that it melted with kelp, so as to make the black glass bottles: which experiment, I have been told, was made by Mr. Dobbs of this kingdom, who is now in London.

Mr.

Mr. Drury, whom I shall have occasion to mention, found in a stone of the Causeway a rough pebble in the shape of an egg, about three quarters of an inch long, and above an inch thick; and when it was polish'd, it proved to be a white cornelian. They are from three sides to nine sides, frequently encompassed with as many stones as there are sides; but many of them have a narrow side, which has no stone to it, but is filled up with a piece or pieces of stone, that shall be further explain'd; which pieces, when the stones are mov'd, commonly separate, and break off. Some stones have two, or three, or more, of these sides: so that it is possible, a stone, that has any number of stones round it, may have double the number of sides: tho' I saw none, that I had reason to think were of this kind, except some, that had probably only three stones round them; being hexagons, with three broad sides, and three very narrow sides.

Whatever the outward figure of the stone is, the concavity or convexity is either circular, or part of a circle; consequently, as the sides of the pillars are plain, the part between the inside circle and the outward figure must either be fill'd up (as it is seen) by stone, which sometimes separates, as mention'd above, and as will be further explain'd; or by the matter pressed up from the sides, as will be more plainly described. In the former case, when the end is convex, this stone often comes off all round at the joint, and leaves the convex end as part of a sphere, and the concave as a mould fitting to it.

I have some stones exactly like a hexagon cut in two, which might be part of a hexagon pillar split;  
for

for sometimes a whole pillar appears as split all the way down ; of which there is a remarkable one at the Causeway.

In relation to the joints in the pillars, this work of nature seems to be different from any thing yet known : and it must be very difficult to assign any satisfactory causes of it.

I submit to the judgment of persons more experienced in these things what has occurred to me.

I suppose, for reasons, which I shall give, that the several parts of these pillars were at first formed either in the shape of a cylinder, with the upper end in a spherical figure, if not both ends ; or that they were either spherical or oblate spheroids.

For, being composed of crystal of six sides, and spar of three, and of a very fine black sand, it may be supposed, that, as the crystals and spars united, and formed an irregular body, the fine black sand fill'd up the interstices, and formed such cylindrical or spherical bodies, as yet soft ; but, in thin horizontal *lamineæ* or plates like talc, as they mostly appear to be ; and, if great force is applied, the stones will separate in such plates between the joints ; and those parts of the pillars, which have been exposed to the weather, and corroded by it, appear in such plates. Sometimes indeed there are perpendicular joints ; as in the split pillar, there seems to have been such a one all down the pillar.

It is therefore probable, that, when this matter was in a fluid state, and when the stratum of rock was formed, on which it was made, the fluid contiguous to the rock still continued in motion ; that, after a time, some of the particles of matter, which  
compose

compose these pillars, being disengag'd from the particles of water, ceas'd to move, and form'd the parts of these pillars, which are next to the rock, in the above-mention'd figures; so much being formed only at once, or in a very short time, as extends to the first joint: that then, either by change of season, or some other accident, so much more water mixed with these particles, as prevented their continuing to form themselves into such a shape, and gave the former motion: that, afterwards, the decrease of the water might again be the cause of the former effect; and so on, till the intire pillars were formed; and the top of the last formed being convex, that, which was formed upon it, would probably be concave, and fit to it, either by its gravity, or by being softer.

All being as yet in some one or other of these figures, we suppose the gravitation of the second stratum above the first joint to operate in such a manner on that which was first formed, and still soft, as to press it down; and so eight stones being round one stone, would naturally press the middle stone into an octagon.

The reasons for concluding, that they were at first in some of these figures, are these:

That the concavity or convexity are either in an intire circle, or part of a circle:

That sometimes the ends of the stones appear to be of a spherical form for some space down, all round the stone; fill'd up only by a matter, that separates from it, as shall be further explain'd.

For it is to be observed, that the pillar is not always so press'd out, as in each stone to form a regular multangular figure; but sometimes there is a  
narrow

narrow side, against which there is no stone, as observed before: sometimes it is pressed out only in part; and this, together with the spherical part, is fill'd up, probably at first with the floating matter; which, I suppose, when the other stone was formed upon it, so united with it, that it remains as a part of the other stone, and breaks off from it, when they are moved: and if this happened to the lower part of the upper stone, this matter, which fills up, might unite with the lower stone; for sometimes this narrow side is seen in the same stone both above and below, the angle being formed in the middle of the stone; and then it is filled up with the matter, which united with the stone above, and the stone below.

It is to be observed, in pursuance of the proof, that the stones were originally round and spherical at the ends; that when the pressure was not sufficient to make out the angles, which I suppose to be the cause of these narrow sides, it is in this case plainly seen, that the original circular shape of the stone is still retain'd; that side not being horizontally in a straight line, but appearing plainly to be part of a circle; as may be seen in the three pieces of stone, which I have sent, that separated from those sides, and fitted into them.

It appears also, that what has been press'd out beyond the circle at the ends is commonly flat, and not concave and convex; as it was probably made, not by the pressure at the ends on the spherical part, but by the pressure on the sides contiguous to it; and when part of the circle is taken off, in that case it is probable, that the pressure on the sides was very great.

In one stone, the matter, which only in part form'd the angle, force being applied to it, came off, and  
 2 left

left that part spherical, being one of those stones, in which one part of the same end is flat, and the other convex, swelling like a cushion.

This stone I sent as a single stone. It is a large octagon, twenty three inches over; but after it had been some time in my garden, I perceived a crack in it, and, applying force, it divided. The under stone had been so unequally press'd, that it is not only very thin on one side, but there is a large hole in it, about seven inches diameter, very near the edge of the stone; so that the matter must have been press'd away to the other side of the stone, not equally concave, and the stone above it must have press'd into the stone below this; in which lower stone the convex part, which press'd through the middle stone, must have been left, as it is broken: which I did not observe at the Causeway.

Some stones at the same end are partly concave, and partly convex; probably occasion'd by such an unequal pressure; so that I have one, which measures nine inches deep on one side, which is convex, and four and a half on the other, which is concave: another, tho' all convex, yet is six inches clear at one angle, and only four at the opposite angle; so that in these stones the joint appears as indented.

We are to suppose, that, generally, the top of the lower stones is convex, and the bottom, consequently, of the stone, that lies upon it, concave. But as sometimes both ends of a stone are concave, we must suppose, either that the lower part of the stone, which settled on it, was harder, or, being of equal hardness, by its gravitation press'd it down.

Since I left the Causeway, I have been inform'd, that commonly, if the top of the stone in a pillar is found either concave or convex, the top likewise of every stone of that pillar is either concave or convex in the same manner; which may be a subject for future observation.

It seems probable, therefore, that all the ends were originally spherical: some of the stones, it may be, exact spheres; others, oblate spheroids; and some longer stones in a cylindrical form, and of a spherical figure, at each end. To which conjecture I have been led, by observing the shape of some I have, and of two models of two stones represented in cork by Mr. Drury, who presented the prospects of the Causeway engraved to the Royal Society, from the drawings of his sister Mrs. Susanna Drury. One of these is convex at both ends; and I have some in the same shape. This spherical figure has been altered by the pressure, in the manner I have observed: for, in the other model, part of the spherical figure is seen round the sides towards the concave end; and I have one exactly of the same kind. In those also, which I have, that are at the same time partly convex and partly concave, the convex part seems to have been the natural figure of the stone, as before described: for, where both ends are concave, that, which was probably press'd by a harder stone form'd before it, is perfectly concave; whereas that concavity, which is made by a stone probably form'd after it, is not so perfectly concave as the other; but it commonly remains convex in some part, as observed before; swelling out like a cushion press'd by any weight.

By



By all that I have been able to observe, the plates seem originally to have been horizontal : in some I have, which are convex, they are apparently so ; and, as far as I could remark, in all, where the plates appear. Tho' it is probable, where the end of the end of the stone is concave, the laminæ or plates have in some measure been press'd in that form ; tho' I could not certainly distinguish it in any of this kind.

Sometimes a joint near perpendicular begins as in a point from the side, and extends into that stone, and into all the stones of the pillar, which are beneath it ; so as (when it has run the length of one stone) to take off either two sides of the stones or pillar, or one side, and part of two sides. This indeed sometimes happens to be in the middle of the pillar, and in the same manner all the way down, so as to form two distinct pillars. Thus I have some, which, by this means, have a side less at one end than at the other ; and I have one, in which the spherical part takes off at one end two sides of the multangular figure, and makes part of a circle, as in some it takes off all the sides at one end ; or, more properly, the stone remains in its original spherical figure. The pieces, which fill up where the stone is not press'd into a multangular figure, sometimes do not break off, as may be seen in the model.

Of the other models made by Mr. Drury, four of them fit to one another, and represent part of a pillar in the Causeway : The seven models not referr'd to, shew a variety of stones ; the measures of all of them are marked in inches, and they are made by a scale of a tenth to an inch. The ends, which are cut smooth in the cork, or are marked with a pencil,

are such, as he could not see, or neglected to observe.

The fourth stone I have sent, which forms part of a circle, broke off from a stone flat at one end, is the spherical part of a stone, such as it appears towards the concave end of one already mention'd.

From these observations, those, who are well versed in natural philosophy, may possibly form some better judgment, and be more happy in their conjectures in relation to this difficult subject, the cause of the joints in the pillars of this extraordinary work of nature.

#### TAB. X.

Represents the plan and profile of the stones brought to Dublin by the Rev. Archdeacon Pococke.

#### *Explanation of the Figures*

Fig. 1. A plan of the pillar, with the measures of the lengths of the sides, *A, B, C, D*.

Fig. 2. A plan of the pillar, with the lengths of the other four sides; *viz. E, F, G, H*, and the distances of the circle from the sides of the polygon.

Fig. 3. A profile of the stones, shewing the sides *A, B, C, D*.

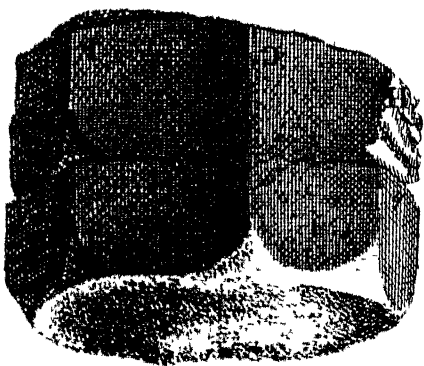
Fig. 4. A profile shewing the sides *E, F, G, H*.

The black lines shew the deviation of the circles from a plane: and the large prick'd waved lines shew the profile of the swelling and concavity within them.

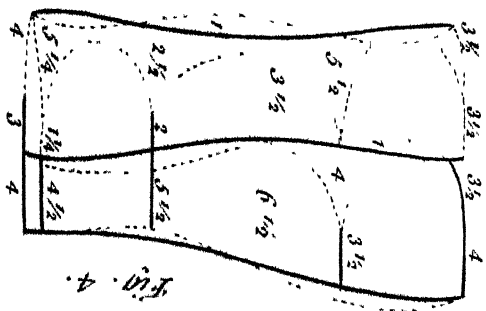
The upper row of figures in each stone shews the heights of the sides at the angles so far as they are strait.

The

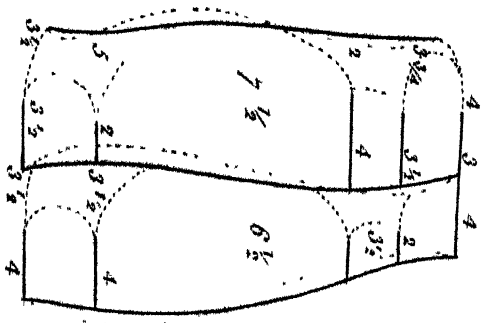




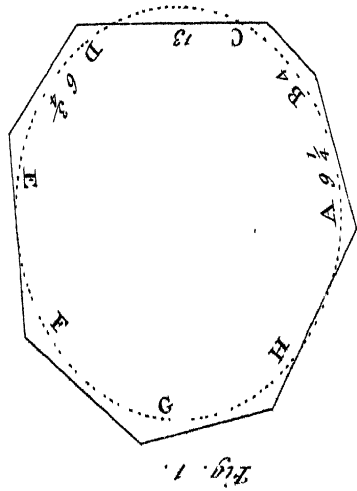
*Fig. 2.*



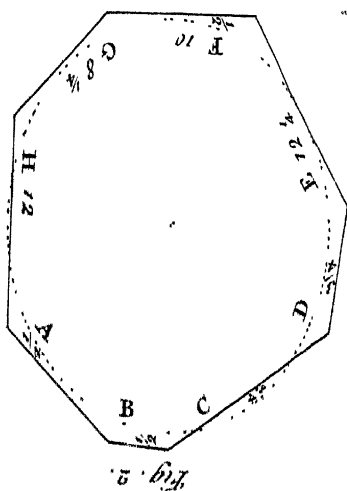
*Fig. 4.*



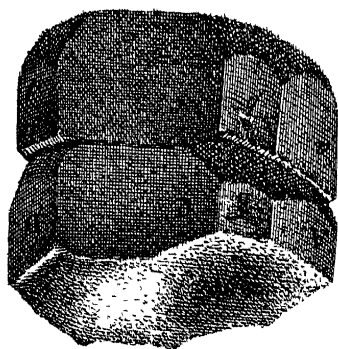
*Fig. 3.*



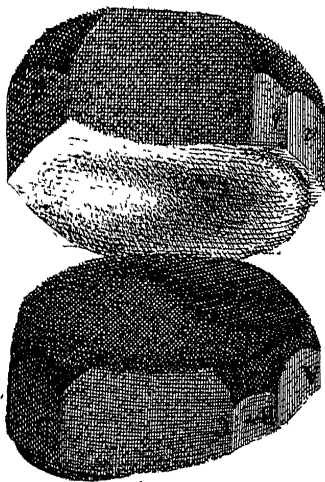
*Fig. 1.*



*Fig. 2.*



*Fig. 5.*



*Fig. 7.*



The under row of figures shews the remainder of the height of the stone at that angle ; or it is the height of the angular curved pieces, which, before they fell off, were the complements filling up the prism, and making the sides of the pillar wholly flat, and the edges or angles of the pillar all strait lines.

Fig. 5. Two upper stones of a pillar, as they stood on the Causeway, shewing 4 sides of the pillar.

Diameter of the upper circle of the upper stone 22 inches.

The circle is about half an inch within the polygon at the side *D* ; but is cut off by the side *C*, about three quarters of an inch.

The sides *B* and *b* a little broken.

Fig. 6. The two stones turned together upside-down ; shewing the other four sides of the pillar.

Diameter of the (now) uppermost circle about twenty-one inches.

The side *H* much broken. Angle *f*, rounded off from the circle to the (now) lower end of the stone. Angle *G* is not rounded off.

Fig. 7. The two stones separated a little to shew the bottom of the upper stone, and top of the under one.

Diameter of the circles, which meet twenty-two inches.

The convex part of the bottom of the upper stone fitting the concave part of the top of the under one ; and the concave part of the bottom of the upper stone fitting the convex part of the top of the under stone.

XXXV. *A Letter upon the same Subject from the Rev. Richard Pococke, LL.D. Archdeacon of Dublin, to the Rev. Tho. Birch, D.D. Secr. R. S.*

Dear Sir,

Dublin, June 18, 1753.

Read June 28,  
1753.

I Received the favour of your obliging letter; and I return my hearty thanks to the Royal Society for their kind reception of the observations, which I sent them on the Giant's Causeway. I am sensible, that it is difficult to make any suppositions on such a subject, that may not be liable to some objections; but I shall be very glad, if mine put any one upon making more happy conjectures.

This day Mr. Drury brought me two maps of the Causeway (*see* TAB. XI.), one a view from the top of the cliff, and the measures as he pac'd; the other from my measures by a line; in which the letters answer to those in the engraving after his sister's drawing. I have sent them to you, as Mr. Folkes some time ago desired to know how far the Causeway extended into the sea.

I have also sent you four drawings by Mr. Drury, of two curious stones, which I brought to Dublin. I am,

Dear Sir,

Your most obedient humble servant,

Richard Pococke.

XXXVI.



XXXVI. *A View of the Relation between the celebrated Dr. Halley's Tables, and the Notions of Mr. De Buffon, for establishing a Rule for the probable Duration of the Life of Man; by Mr. William Kerffseboom, of the Hague. Translated from the French, by James Parsons, M. D. and F. R. S.*

— eo disconvenit inter

*Meque et te* —

Read May 31, 1753. § 1. MY situation in life not permitting me to look over the works of the learned, was the reason, why I was not one of the first, who perused the *General and particular Natural History*, &c. of Mr. de Buffon. However a little interval of leisure allowing me to look into it, I am at a stand to find myself mention'd, in the same breath with the celebrated Dr. Halley and others, to receive our condemnation on account of the tables for determining the degrees of probability of the duration of human life: and as this passage is the occasion of my remarks, I will begin by citing it intirely here:

“ Man (says M. de Buffon, at the end of the  
 “ second tome) as is well known, dies at all ages;  
 “ and altho' it may be said in general, that his life  
 “ is longer than that of almost any other animal, it  
 “ cannot be denied, that it is at the same time more  
 “ variable and uncertain. Attempts have been of late  
 “ years made to know the degrees of these variations,  
 “ and

“ and to establish, by observations, some certainty  
 “ concerning the mortality of mankind of different  
 “ ages. If these observations were sufficiently exact,  
 “ and a sufficient number of them made, they would  
 “ be of very great use towards knowing the number  
 “ of the people, of their increase, of the consumption  
 “ of provisions, of the division of taxes, &c. Many  
 “ ingenious men have studied this subject; and lately  
 “ Mr. Deparcieux, of the Academy of Sciences, has  
 “ given us an excellent work, which serves as a rule  
 “ for the future, with respect to annuities for life:  
 “ but, as his principal view was to calculate the mor-  
 “ tality of annuitants, and that, in general, annuitants  
 “ for life are men in one state, no conclusion can be  
 “ drawn from it for the mortality of mankind at  
 “ large. The tables, which he has given in the  
 “ same work upon the mortality of the different re-  
 “ ligious orders, are also very curious; but, being con-  
 “ fined to a certain number of men, who live in a  
 “ different manner from others, they are not yet suf-  
 “ ficient to found exact probabilities with relation  
 “ to the general duration of life.

“ Dr. Halley, Messieurs Graunt, Kerseboom,  
 “ Sympson, &c. have also published tables of the  
 “ mortality of mankind; and they have founded  
 “ them upon extracts from the bills of mortality of  
 “ some parishes of London, Breslaw, &c. But it ap-  
 “ pears to me, that their researches, however ample,  
 “ and the result of long study, can afford only very  
 “ distant approaches to the knowledge of the mor-  
 “ tality of mankind in general. In order to make a  
 “ good table of this kind, not only the registers of  
 “ the parishes of such a city as London, Paris, &c.  
 “ should

“ should be made use of, where foreigners are daily  
 “ coming in, and natives going out, but also those  
 “ of the country; that, by adding together the re-  
 “ sults of each, the one may compensate the other.  
 “ M. Dupré, de St. Maur, of the French Academy,  
 “ has begun this upon twelve country parishes, and  
 “ three of those of Paris: he was willing to commu-  
 “ nicate these tables to me to publish them; and I  
 “ the more readily do it, because they are the  
 “ only tables, upon which the probabilities of the  
 “ life of mankind in general can be establish’d with  
 “ any certainty.”

As for the tables, I refer for them to the work.

§ 2. I am not at all concern’d for a defence of my  
 table, which is sufficient to support itself; but I am  
 greatly surpris’d, that a philosopher should condemn  
 works, which he never either saw or read: for it is  
 evident, M. de Buffon never saw my *Essays on politi-  
 cal Arithmetic*; and that all, which he appears to  
 know of it, is indeed very slightly drawn from M.  
 Deparcieux’s work, who nevertheless knew no more  
 of it, as he himself makes it appear, than what he  
 found in the *Bibliothèque raisonnée* for the first three  
 months of the year 1743, Tom. 30. This extract  
 happens unluckily not to be made by an able hand;  
 but, on the contrary, very fit, by its confusion, and  
 the irregularities, which run thro’ it, to lead into  
 errors. The corrections, that were made in the second  
 part of the same 30th Tome, are not even sufficient  
 to secure the reader from mistakes.

Nevertheless M. de Buffon, without even reading  
 the work, might have known more of it, though

H h

written

written in a language, which he is very likely a stranger to; since Mr. Eames has given an excellent extract of the first essay in English, printed in N<sup>o</sup> 450 of the *Transactions* of the Royal Society of London, of which this philosopher is a member; and also because Mr. Van Rixtel has inserted an extract of the two succeeding ones in N<sup>o</sup> 468. of the said *Transactions*. Besides which, M. de la Chapelle has given, in his 13th Tome of the *Nouvelle Bibliothèque* for the month of December in the year 1742, a very ample and judicious extract of the three essays together. I have also confirm'd my observations by a very interesting and sensible proof, upon that part of London called the city, which is printed in the first three months of the year 1743, Tom. 14. of the *Nouvelle Bibliothèque*; and likewise in the second three months of the year 1745 of the *Bibliothèque raisonnée*, is inserted. a small piece relative to my observations and proofs. I take the liberty of referring every reader, who may not understand the Low-Dutch language, to those several pieces cited; and my table of the degrees of the probability of the duration of the life of man will support itself very well against the hasty judgment of M. de Buffon, who certainly has too much candour not to acknowledge, after a mature deliberation of those pieces mention'd, that they contain something more than "very distant approaches to the knowledge of the mortality of mankind in general."

§ 3. I would say much the same of that excellent piece of the learned Dr. Halley, if my surprize did not increase, the more I reflect, that this work ought to be thoroughly known to a member of the Royal Society of London; and that this very member nevertheless makes so careless a judgment upon

it. This reflection leads me to another kind of defence of that famous deceased author; which is to make M. de Buffon sensible, that “ nearly the same degrees of probability of the duration of the life of “ man in general” are in the table of Dr. Halley, which he would have us think are in the extracts of M. Dupré’s observations or tables, which he has published. For this purpose I constructed a table parallel to that of Dr. Halley, which begins with 1000 lives of one year old, and which I found, in the reduction of the great general numbers of Dupré’s tables, to have also the smaller numbers analogous; that is, by beginning also with 1000 lives of a year old. Both tables are laid down as follows:

Halley’s Table.			Dupré’s, reduced.		
Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.	Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
1	1000		1	1000	
2	855	145	2	864	136
3	798	57	3	808	56
4	760	38	4	768	40
5	732	28	5	739	29
6	710	22	6	716	23
7	692	18	7	699	17
8	680	12	8	685	14
9	670	10	9	676	9
10	661	9	10	670	6
11	653	8	11	665	5
12	646	7	12	659	6
13	640	6	13	655	4
14	634	6	14	651	4
15	628	6	15	646	5
16	622	6	16	640	6

Halley's Table.

Dupre's, reduc'd.

Years of Age	Number of Lives	Number of Deaths fr. Year to Yr	Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
17	616	6	17	634	6
18	610	6	18	628	6
19	604	6	19	622	8
20	598	6	20	614	5
21	592	6	21	609	10
22	586	7	22	599	7
23	579	6	23	592	7
24	573	6	24	585	11
25	567	7	25	574	8
26	560	7	26	566	8
27	553	7	27	558	8
28	546	7	28	550	6
29	539	8	29	544	13
30	531	8	30	531	5
31	523	8	31	526	10
32	515	8	32	516	8
33	507	8	33	508	7
34	499	9	34	501	16
35	490	9	35	485	10
36	481	9	36	475	9
37	472	9	37	466	9
38	463	9	38	457	5
39	454	9	39	452	23
40	445	9	40	429	5
41	436	9	41	424	11
42	427	9	42	413	7
43	417	10	43	406	6
44	407	10	44	400	18

Halley's

## Halley's Table.

## Dupré's, reduced.

Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.	Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
45	397		45	382	8
46	387	10	46	374	6
47	377	10	47	368	9
48	367	10	48	359	6
49	357	11	49	353	21
50	346	11	50	332	5
51	335	11	51	327	9
52	324	11	52	318	5
53	313	11	53	313	7
54	302	10	54	306	4
55	292	10	55	302	19
56	282	10	56	283	7
57	272	10	57	276	11
58	262	10	58	265	5
59	252	10	59	260	30
60	242	10	60	230	5
61	232	10	61	225	10
62	222	10	62	215	9
63	212	10	63	206	13
64	202	10	64	197	13
65	192	10	65	184	13
66	182	10	66	171	7
67	172	10	67	164	13
68	162	10	68	151	6
69	152	10	69	145	22
70	142	11	70	123	6
71	131	11	71	117	15
72	120	11	72	102	7

Halley's

Halley's Table.

Dupre's, reduc'd.

Years of Age	Number of Lives	Number of Deaths fr. Year to Yr	Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
73	109	11	73	95	9
74	98	10	74	86	15
75	88	10	75	71	6
76	78	10	76	65	7
77	68	10	77	58	9
78	58	9	78	49	3
79	49	8	79	46	14
80	41	7	80	32	3
81	34	6	81	29	5
82	28	5	82	24	4
83	23	3	83	20	3
84	20		84	17	

Sum total of Dr. Halley's table . . . 34000

Sum total of M. Dupré's table reduced . . . 33911

In the whole matter, all the difference between these two tables consists in this, that Dr. Halley's is more perfect, more compact, and more conformable to those observations, which conduct us all to the idea of a progression nearly arithmetical, which the great number of researches enables us to unfold by little and little, in the representation of the strength of human life, when that strength is become more uniform.

§ 4. If M. de Buffon will compare the table given by himself of the probability of the duration of life, which is founded upon that of M. Dupré de St. Maur, with



with that given by M. Deparcieux, in his ingenious work constructed upon that of Dr. Halley, he will find a like conformity between them. It is M. Deparcieux's thirteenth table, which I have before me. What follows, in Halley's column, I set down, in order to compare it with M. de Buffon's table.

Deparcieux upon  
Halley.

De Buffon upon Dupré  
of St. Maur.

Years of Age	Lives at a Me- dium.		Years of Age	Duration of Life.	
	Years	M <sup>os</sup> .		Years	Months
1	33	6	1	33	
2	38	0	2	38	
3	39	9	3	40	
4	40	9	4	41	
5	41	3	5	41	6
6			6	42	
7			7	42	3
8			8	41	6
9			9	40	10
10	40	5	10	40	2
11			11	39	6
12			12	38	9
13			13	38	1
14			14	37	5
15	37	6	15	36	9

Deparcieux

Déparcieux upon  
Halley.

De Buffon upon Dupré  
of St. Maur.

Years of Age	Lives at a Medium.			Years of Age	Duration of Life.	
	Years	Months			Years	Months
16				16	36	0
17				17	35	4
18				18	34	8
19				19	34	0
20	34	2		20	33	5
21				21	32	11
22				22	32	4
23				23	31	10
24				24	31	3
25	30	11		25	30	9
26				26	30	2
27				27	29	7
28				28	29	0
29				29	28	6
30	27	11		30	28	0
31				31	27	6
32				32	26	11
33				33	26	3
34				34	25	7
35	25	0		35	25	0
36				36	24	5
37				37	23	10
38				38	23	3

Déparcieux

Deparcieux upon  
Halley.

De Buffon upon Dupré  
of St. Maur.

Years of Age.	Duration of Life.			Years of Age.	Duration of Life.	
	Years.	Months.			Years.	Months.
39				39	22	8
40	22	4		40	22	1
41				41	21	6
42				42	20	11
43				43	20	4
44				44	19	9
45	19	8		45	19	3
46				46	18	9
47				47	18	2
48				48	17	8
49				49	17	2
50	17	3		50	16	7
51				51	16	0
52				52	15	6
53				53	15	0
54				54	14	6
55	14	10		55	14	0
56				56	13	5
57				57	12	10
58				58	12	3
59				59	11	8
60	12	5		60	11	1

Deparcieux upon  
Halley.

De Buffon upon Dupré  
of St. Maur.

Years of Age.	Duration of Life.		Years of Age.	Duration of Life.	
	Years.	Months.		Years.	Months.
61			61	10	6
62			62	10	0
63			63	9	6
64			64	9	0
65	9	11	65	8	6
66			66	8	0
67			67	7	6
68			68	7	0
69			69	6	7
70	7	7	70	6	2
71			71	5	8
72			72	5	4
73			73	5	0
74			74	4	9
75	5	7	75	4	6
76			76	4	3
77			77	4	1
78			78	3	11
79			79	3	9
80	4	6	80	3	7
81			81	3	5
82			82	3	3
83			83	3	2
84	3	6	84	3	1
					6 5

§ 5. It is therefore sufficiently demonstrated, that Dr. Halley's table ought not, in M. de Buffon's opinion, to be excluded from the class of those, which "are the only tables, upon which the probabilities of the life of mankind in general can be established with any certainty;" far from being comprised, in his severe judgment, among those of authors, "whose researches, however ample, and the result of long study, can afford only distant approaches to the knowledge of the mortality of mankind in general."

§ 6. M. de Buffon begins his table of the probabilities of life with a term, which precedes that of a year old, called *zero d'âge*; and from M. Dupré's observations, assigns to it a duration of 8 years. I first thought it an error of the press; but there is no room for this doubt, after what M. de Buffon says, "We see by this table (says he) that one may reasonably hope, that is, lay an even wager, that an infant just born, or who has *no age*, will live eight years: that an infant, who is a year old, will live thirty-three years," &c. This little space of eight years struck me; because all the observations, which I know, are very far from it. I had therefore recourse to the source, to the observations of Mr. Dupré himself, and I found it was a mistake of M. de Buffon; the mean life of infants of *no age* being, according to M. Dupré's tables, twenty-and-five years and upward; and, from the observations of Justel, which Dr. Halley made use of, the mean life of a child of *no age* is above twenty-and-seven years.

§ 7. I might make an end here, if the subject did not absolutely require my offering a word concerning the nature of both Justel's and M. Dupré's observations.

The remark has not escaped the sagacity of Dr. Halley himself: it is, “that they want the essential;” which is, the number of living persons, among “whom the observations upon the dead are made.” If M. de Buffon had made the same reflections upon M. Dupré’s tables, he would have found the irreparable defect of them, as well as Dr. Halley did in Justel’s observations; and he would have attended more, without doubt, to the method proposed by Monf. Deparcieux.

§ 8. I shall put an end to this treatise by a word or two, which relates to Dr. Short’s displeasure against me. This may perhaps be unnecessary, after the laborious and judicious extract of his book in the *Journal Britannique*, of the month of July 1750. However since Dr. Short joins with those, who inconsiderately have accused me of partiality to such or such a city, he will have with them my defence (to say no more) in my letter to Mr. Eames, and in the piece already quoted in the *Nouvelle Bibliothèque*, Tom. XIV. Artic. 8. Let him read, and he will return from his prejudice: if not,

*Curentur dubii medicis majoribus ægri.*

Hague, May 11,  
1753.

Wm. Kerseboom.

XXXVII. *A Letter from Father d'Incarville,  
of the Society of Jesus, at Peking in China,  
to the late Cromwell Mortimer, M. D.  
R. S. Secr.*

SIR,

Pekin, Nov. 15, 1751.

Read June 7,  
1753

I SHOULD be glad to have it in my power to do more for your illustrious Society, both as to my situation and condition. We are so much confin'd at Peking; we have not even the liberty of going where we please by ourselves to see things; nor can we, with prudence, believe the reports of the Chinese, who make nothing of deceiving us, if they can defraud us of our money. When we can do no otherwise, we endeavour to prevent our becoming their dupes, as well as we can: and yet, notwithstanding all our precautions, we cannot answer for what intelligence we have this way, so well as for what we affirm to have seen ourselves. Every year I send to our gentlemen of the Academy at Paris what I can discover upon such matters, as I know concern them; which has given me the opportunities of sending you several curious things in natural history.

You ought to have received last year some leaves and flowers of different trees, besides a good number of seeds. The leaves and flowers of the varnish-tree, which I sent, come from the province of Nan King. This tree is different from that I saw in the king's garden at Paris. The latter is the same with  
what

what I saw at Macao; which was brought from Mississippi into France.

We have not in Europe the tree, from whose fruit the Toeng yeou is drawn. It were to be wish'd they could raise it there. The Toeng yeou is an oil, or natural varnish, drawn by expression from the fruits, which I have sent you, of which they make a very great trade in China. It costs but very little, the pound weight being worth about 7 or 8 sols of our money. I heard say, that they sell it at Paris under the name of China varnish. It is excellent for preserving furniture, giving them a polish not inferior to our varnishes of Europe, which cost so much money. Perhaps they may make some attempts to use it in Europe; but they will not succeed, because they know not how to prepare it. This oil is so common in China, that the greatest part of the people, in tolerable circumstances, rub over their timber with it, giving it what colour they please. It not only adorns their houses, but also preserves the wood. The columns, that support their houses, and those of the great room where the emperor's throne is, are varnish'd with no other than this oil.

The Kou chou is a tree, of the bark of which they make the best paper in China. The common paper of their books, which looks yellowish, is made of particular species of Bambou, of which they prepare the young shoots, as we prepare hemp. They whiten it, by boiling it in lime water: in this manner they prepare the Kou chou. There is no filken paper in China; all the different kinds of paper here are made either of bark, hemp, or of the straw of corn or rice. Sometimes they blend with this last the  
stalks



stalks of the *Typha* \*. The paper made of hemp or straw serves only for wrapping up goods, or to make pasteboard; and that made of the bark of the cotton-plant serves for fans, being less apt to crack than any other white paper.

The white wax, produced by certain insects, is a very curious and profitable thing. I have not yet been able to see any of them. What has been told me by one of our missionaries, who has bred them himself, is not sufficient to give a proper idea of them. As to the manner of their depositing this wax, it appears to me, that there is some analogy between it, and the manner of the gum lac's being deposited by certain ants †.

In the emperor's palace they very rarely use any other candles, than such as are made of this wax, because it never emits any smoke. The learned therefore use them only, when they compose an exercise upon their examination for degrees: for then they are confined in very small rooms, where the smoke of tallow-candles would incommode them greatly. I believe the chief consumption of this wax is owing  
to

\* *Typha palustris* major of Caspar Bauhin. Cat's-tail.

§ In order to explain this passage, I take the liberty of making the following remark. The Lacca-tree is the *Fujuba Indica* of the great Ray; which produces this gum. The letter-writer is misled by what Garcias ab Horto says about it, that certain large-wing'd ants make this gum out of the juice suck'd from this tree, and deposit it upon the *furculi*, &c. of the same: but the celebrated Ray and J. Bauhin say, it is exudated, and by the heat of the sun concentered into the form, in which it is found upon the parts of this tree. There are other trees, which produce this gum, as well as this, mention'd by Hermannus.

to their coating tallow-candles with it, which I shall mention by-and-by. This wax is procured by boiling the matter rasped off the branches of the tree, the leaves of which are the proper nourishment of these insects, in a large vessel of water; the wax swims at the top, and, when cold, it is taken off in a cake.

The berries of the tallow-tree are of great use in the southern provinces, where there are very few sheep. Almost all the candles, sold there, are made of the oil drawn from these berries. They procure this oil in the same manner, that I have mention'd concerning the wax; and as this oil is not of so good a consistence as tallow, for its cohesion, when candles are made of it, they dip them in the white wax mention'd: the external coat, thus made, prevents them from guttering \*. At Peking the same thing is done with tallow-candles; nor do I ever remember to have seen them run down. I imagine, that our bees-wax would answer the same purposes with this white wax of China.

The seeds of the Yen tchi come from a plant, which I think very particular; at least I cannot recollect any thing like it. From these seeds or berries, when very ripe, a tincture of a fine red is drawn, as may be seen in the flakes of cotton charged with this colour, sold here. They moisten them with a little warm water, and then express the colour, which is afterwards evaporated to a driness, and serves for water-colours.

The

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\* This is applicable to the green wax of Mississippi.

The *Perficaria*, of which they make indigo in and about Peking, merits attention. Indigo is also made of the *Perficaria maculata*, with which the banks of rivers and streams often abound; but it is of an inferior quality to that made with the other *Perficaria*, the seeds of which I sent you; and this even is not of equal value with that made of the anil, such as is made in the southern provinces here, and in those of America.

The stones of apricots come from a species of tree, whose fruit is not eatable. These trees are only cultivated for these stones, from which an excellent oil is produced for burning; and which, instead of olive-oil, we use for our fallads.

The *Hoai tze* are the clusters of the flower of a bastard Acacia, from whence a most beautiful yellow tincture is drawn, by boiling them with a little alum. The *hoang tchi tze* produces yet a finer tincture: but the finest yellow colour of China comes from the *hoang pe pi*; and these three are prepared in the same manner.

A kind of stuff is made from the cods of the wild silk-worm, called *kien tcheou*, excellent for wear, when made for gain, but chiefly that, which is made from such cods, as I sent you in 1749. It is scarce, and dear. There is another kind of *kien tcheou*, of which they sell a large quantity at Canton: it is made of the silk drawn from other cods, some of which I send you this year. These cods are capable of being wound on wheels or spindles. The first I sent are only wound on spindles; but first they must be boiled in a strong lye, made of the ashes of the stalks of the *Sarazin* corn, till they are capable of

being pull'd aſunder with one's fingers, in order to turn them inſide-out, and take out the fragments of the chryſalis ; and as this kind of ſtuff is work'd like other cloth, the weavers do the reſt.

The fruits of the *tong yeou*, and of the tallow-tree, which you ſhould have received laſt year, were freſher than thoſe I ſent before.

This year you will receive the cods of ſilk, which makes the ſilk called *kien tcheou*, with the butterflies, which come from them. The other things, which I ſent, want no explanation.

*An Answer to the Questions upon the Natural Hiſtory of Foffils.*

THE empire of China abounds in mines of all ſorts, as gold, ſilver, copper, tin, lead, iron, &c. The provinces, which produce the greateſt quantity, are *Yun nan*, and *See tchouen*. The two greateſt rivers of China, *Kiang*, and *Hoang ho*, ſend down quantities of gold ſand. The former takes its ſource in the province of *See tchouen*, and the latter from *Coconor* : but they find mines of gold and ſilver in the provinces of *Yun nan*, *See tchouen*, *Chen ſi*, *Chan tong*, *Hou kouang*, *Fou kien*, *Kouei tcheou*, *Pe tche ſi* ; but, for political reaſons, they work but few of them. I believe the principal is, leſt the greedineſs of gain ſhould excite popular inſurrections. They open them ſometimes in one place, ſometimes in another : but, upon the leaſt appearance of a riſing, they immediately ſhut them up again. We cannot give any account of what is deſired, concerning the manner of working the ſeveral mines. We are not  
in

a way of informing ourselves. I have endeavour'd for several years to procure specimens of the different mines, but could not yet obtain them. If, hereafter, I can discover any thing worth while in this matter, I shall not fail to communicate it. As to what regards petrifications, I have only seen a few crabs, pieces of wood, and some bones, which I take to be those of buffaloes. I have sent into France specimens of all the simple drugs sold by the druggists at Peking; among which are some bits of minerals, petrified bones, &c. to which I expect an answer next year, and shall be better able to chuse what to send of such things, as shall be desired. This collection is one of the affairs, that cost me most trouble.

The article, that regards the deluge, makes me imagine, that the list of these things comes from the celebrated Sir Hans Sloane. I should be glad to have an opportunity of doing him pleasure, and I would do it most readily. All I know of it is this; the Chinese have but a very confused idea of an universal deluge. They only conclude from things seen upon the surface of the earth, that there must formerly have been some terrible hurricane, and that the sea had cover'd the face of the earth. A great mandarin, who had a better understanding than the Chinese commonly have, being sent into *Ho nan*, to visit several places, observed, upon the top of a very high mountain, a kind of basin, the circumference of which, formed by the mountain, was filled with different figures of fishes, shells, and marine plants, impressed upon stones: He said to another mandarin, who accompanied him, "Certainly the sea must have been here: these fishes, shells, and, plants are

“ found only in the sea.” F. Gaubil says, the Chinese books pretend, that such impressions are found upon the highest mountains of *Tibet*, and *See tchouen*. I had an opportunity myself to go into the mountains about Peking, and even went up to the highest, but saw nothing of this kind; and was informed upon the spot, that they never found any thing like them.

The greatest part of the cinabar of China comes from the province of *Yun nan*: and it is said, there is some also in *Kiang si*, *Hou kouang*, and *Koui tcheou*. *Kang hi*, the great-grandfather of the present emperor, ordered a general search to be made thro’ the whole empire for antimony, but found none in any of the mines.

I have the honour to be, with much respect and esteem,

S I R,

Peking, Nov. 15,  
1751.

Your most humble and

obedient servant,

D’Incarville.

XXXVIII. *A Letter from Mr. T. Melvil to  
the Rev. James Bradley, D. D. F. R. S  
With a Discourse concerning the Cause of  
the different Refrangibility of the Rays of  
Light.*

S I R,

Read March 1, 1753. THO' I have not the honour of your acquaintance, I have presumed to address the inclosed paper to you, relating to optics and astronomy. The unwearied zeal which you have shewn, Sir, for the improvement of these sciences, made me conclude, that whatever has an appearance of truth or novelty on such subjects, tho' inconsiderable in itself, might not be unacceptable to you. Besides, among all the societies of the learned in Europe, I could think of no abler judge to consult on these matters, than the author of the "Aberration of Light, and of the Variation of the Precession of the Equinoxes:" the two greatest discoveries, without doubt, that have been made in astronomy for half a century. Perhaps you may be able, Sir, from the many accurate observations you have already made, to decide the question relating to the velocity of light; or, at least, to determine, whether the observation proposed be practicable: for I am very sensible, that many observations, which appear easy in speculation, cannot be put in execution.

If you find any thing in the inclosed worthy of the public view, you are at liberty to lay it, in whole, or  
in

in part, before the Royal Society, or to make what use of it you please. I am,

S I R,

Geneva, Feb. 2,  
1753.

Your most obedient humble servant,

T. Melvill.

*Concerning the Cause of the different Refrangibility of the Rays of Light, by Mr. T. Melvill.*

Read March 8, 1.  
1753.

**I**N order to account for the different refrangibility of the differently-colour'd rays, Sir Isaac Newton†, and several of his followers, have supposed, that their particles are of different magnitudes or densities; but if there be any analogy between the refractive power and gravity, it will produce equal velocities in all particles, whatever their magnitude or density be; and so all sorts of rays would be equally bent from their right-lined direction.

2. It seems therefore a more probable opinion, which others have advanced, that the differently-colour'd rays are projected with different velocities from the luminous body; the red with the greatest, violet with the least, and the intermediate colours with intermediate degrees of velocity; for, on this hypothesis it is manifest, that they will be differently refracted

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† Newton's Optics, Query 29.



refracted in the prismatic order, according to observation.

3. On supposition, that the different refrangibility of light arises solely from the different velocities of the rays before incidence, these velocities must be to one another nearly as their sines of refraction. — I say nearly; for their exact proportion cannot be discovered, but by the solution of the following problem, which I take this opportunity of proposing to the learned:

If two bodies fall, in equal angles of incidence, on a space terminated by parallel planes, in which any power acts perpendicularly to the planes (according to the hypothesis in Prop. 94, Lib. I. of the *Principia*) the ratio of the sines of emergence to the common sine of incidence, and consequently to one another, being given, to determine the proportion of their velocities at the time of their incidence on the first plane.

4. Their velocities in any given medium (suppose air) being once determined, their velocities in any other may be easily discover'd; for they are to those in air as the sine of incidence to the sine of refraction, when the ray passes from air into the other medium †.

5. While the differently-colour'd rays are supposed to move with one common velocity, any pulses, excited in the æthereal medium, must overtake them at  
equal

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† *Principia*, Phil. Nat. Prop. 95, Lib. I.

equal distances; and therefore the intervals of the fits of reflexion and transmission, if they arise in this manner, as Sir Isaac conjectures, would be all equal: but if the red move swiftest, the violet slowest, and the intermediate colours with intermediate velocities, it is plain, that the same pulses must overtake the violet soonest, the other colours in their order, and last of all the red; that is, the intervals of the fits must be least in the violet, and gradually greater in the prismatic order, agreeably to observation.

6. Let  $c$  denote the velocity of the æthereal pulses,  $V$  the velocity of red light, and  $U$  that of violet;  $I$  and  $\mathcal{J}$  the intervals of their fits, and  $D$  the distance between two succeeding pulses: it is plain, from the nature of Newton's hypothesis, that  $I$  is to  $D$ , as  $V$  to  $C - V$ : and again,  $D$  to  $\mathcal{J}$  as  $C - U$  to  $U$ : therefore, *ex æquo*,  $I$  is to  $\mathcal{J}$ , as  $CV - VU$  to  $CU - VU$ , from which we have the equation  $C = \frac{I - \mathcal{J} \times VU}{I \times U - \mathcal{J} \times V}$

Therefore, as the proportion between the intervals of the fits in red and violet, can be assigned by experiment, and the proportion of their velocities in any medium likewise, by Art. 4. the velocity of the æthereal pulses may be easily computed. The velocities of the red and violet in air are nearly as 78 and 77. In the celestial spaces they are less, but almost in the same proportion; the intervals of their fits are by experiment as 100 and 63 †, from whence, by the canon now laid down, the velocity of the æthereal pulses in the celestial space is found to be to that of red

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† Newton's Optics, Lib. II. Part I. Obs. 14.

red light as 79763 to 78000. As light moves from the sun to us, by Dr. Bradley's accurate estimation, in 8' 12" (a), the pulses of the æthereal fluid must be propagated thro' the same space in about 8' 1".

7. Hence also may be determined, in known measures, the distance between two succeeding æthereal pulses for  $D = \frac{CI - VI}{V}$ .

8. Upon the hypothesis of the different velocities of different colours, we may understand, at least in general, the reason of the strange analogy, discover'd by Sir Isaac, between the intervals of the fits, and the spaces occupied by the several colours in the spectrum (a thing hitherto unexplained (b); since, from the velocities of the several rays, upon which depend the intervals of the fits, as has been now explained, arise likewise their several degrees of refrangibility.

9. But, as it is of great consequence in philosophy, to distinguish between facts and hypotheses, however plausible, I observe, that the various refrangibility, reflexibility, and inflexibility, of the different colours, and their alternate dispositions, at equal intervals, to be reflected and transmitted, which are the whole ground-work of the Newtonian system, are to be consider'd as undoubted facts, deduced from experiment; but that the velocities of different rays are different in the manner now describ'd, is no more than probable conjecture: and tho' this point should be decided, by a method that we are now to propose,  
it

(a) Eames's Abridg. Transact. Vol. VI. p. 157.

(b) Compare Newt. Opt. Book I. Part 2. Prop 3. with Book II. Part 3. Prop. 16.

it would still remain uncertain, whether the fits of reflexion and transmission consist in an alternate acceleration and retardation of the particles of light, or in something else. For instance, it might be supposed, that every particle of light has two contrary poles, like a loadstone; the one of which is attracted by the parts of bodies, and the other repell'd; and that, besides their uniform rectilinear motion, the particles of differently-colour'd rays revolve in different periods round their centers; for thus their friendly and unfriendly poles being alternately turned towards the surfaces of bodies, they might be alternately disposed to reflexion and transmission, and that at different intervals, in proportion to the periods of their rotation. Lastly, tho' it were proved, that the fits do proceed from an alternate acceleration and retardation of the particles of light, it would still be no more than probable conjecture, that this is brought about by pulses excited in the æthereal medium. Nay there are some circumstances in these phænomena, that seem hardly intelligible by that hypothesis alone: as, Why the intervals of the fits are less in denser mediums (*a*); and, Why they increase so fast, and in so intricate a proportion, according to the obliquity of incidence (*b*).

10. By Dr. Bradley's beautiful theory of the aberration of light, the stars appear to be removed from their true place to a certain distance, according to the proportion which the transverse motion of the spectator's eye bears to the velocity of light. It is plain therefore,

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(*a*) Newt. Opt. Book II. Part 3. Prop. 17.

(*b*) Prop. 15. *ibidem*.

therefore, that, on our hypothesis, any star must have a different apparent place for every different colour; that is, its apparent disk must be drawn out by the aberration into a longitudinal form, resembling the prismatic spectrum, having its red extremity nearest its mean place. In the stars situated about the pole of the ecliptic, its length should continue always the same, tho' directed along all the different secondaries of the ecliptic in the course of a year: but in those which lie in or near the plane of the ecliptic, it should be greatest at the limits of the eastern and western aberrations, the star recovering its colour and figure, when the true and mean places coincide. But there is no hope of discovering, whether our system be true or false, by this consequence of it: for the greatest length of the dilated disk being to the whole aberration, as the difference of the velocity of the red and violet to the mean velocity of light, *i. e.* but about a 77th part of it, cannot much exceed the fourth part of a second.

11. The time which the extreme violet light takes in arriving from any distance to the eye, will be to that which the extreme red takes in coming from the same, as 78 to 77. If Jupiter be supposed in a quadrature aspect with the sun, in which position the eclipses of his satellites are most commodiously observed, his distance from the earth being nearly equal to his distance from the sun, light takes about 41' in passing from him to the earth; therefore the last sensible violet-light, which the satellite reflects before its total immersion into Jupiter's shadow, ought to continue to affect the eye for a 77th of 41'; that is about 32" of time after the last sensible red light is gone. It is therefore a certain consequence of our

hypothesis, that a satellite, seen from the earth, ought to change its colour about half a minute before its total immersion, from white to a livid greenish colour; thence into blue, and at last vanish in violet. I need hardly observe, that the same phenomenon must take place in the time of emergence by a contrary succession of colours, beginning with red, and ending in white.

12. If this phenomenon be perceived by astronomers, we shall have a direct proof of the different velocities of the differently-colour'd rays, and, consequently, a mechanical account of their different degrees of refrangibility; for I see not, to what other cause such an appearance could be reasonably ascribed. If it be not, we may conclude, that rays of all colours are emitted from the luminous body with one common velocity.

Geneva, Feb. 1, 1753.

T. Melvill.

This paper was delivered to Mr. Short, in order to attend to the particular observation of Jupiter's satellites recommended by Mr. Melvill, who after some time made the following report to the Society.

EVER since this paper of Mr. Melvill's was put into my hands, I have carefully attended the emergences of Jupiter's first satellite thro' a reflecting telescope of four feet focal length, and with a proper magnifying power; but I have not perceived the least alteration in the colour of the light reflected by the satellite;

satellite, except in quantity. It may indeed be observed, that these emerfions are seen fooner or later thro' telescopes of different lengths, and by eyes of different goodnefs: and it may therefore be alleged, that there is a certain quantity of time elapsed between the very first emerfion of the satellite, and the instant when it is perceived by the very best eye; assisted by the best telescope; and that, during this interval, the succeffion of colours, above-mention'd, is perform'd. But our author, in consequence of his hypothesis, says, that this succeffion of colours may be perceived for the space of 32" after the first emerfion of the satellite; and I am fully satisfied, from repeated observations, that the quantity of time elapsed from the very first emerfion of the satellite, till it is perceived by a good eye, assisted by a good telescope, can amount only to a very few seconds. So that, upon the whole, we may conclude, that it does not appear, by the observations of the emerfions of the first satellite of Jupiter, that the rays of different colours move with different degrees of velocity.

But our author's conclusion, that, if the rays of light emitted from Jupiter's satellites, at the time of their immersion and emerfion, should not be found of different colours, the rays of all colours emitted from luminous bodies will have one common velocity, seems only to hold good, on a supposition, that light is propagated by a continued motion, in the manner of a projectile.

Dr. Knight, in his treatise on attraction and repulsion (Prop. 69.) has consider'd the propagation of light, as performed by vibrations in an elastic fluid, in the same manner as sound is produced by vibration

vibrations in the air: and he thinks, that it is as easy to conceive how the velocities of the particles of light may be different, and yet take up equal times in propagating their motion from one to another through a given space, as to explain how sounds of different tones move with equal velocities. In accounting for both, he shews, that, in a series of particles, which mutually repel each other, the greater their velocity, the nearer they will approach other, in communicating their motions from one to another; and consequently each of them must move thro' a greater space in so doing: wherefore the same time may be spent in propagating a successive motion thro' a series of particles, whose velocity is greater, if each particle has to move thro' a greater space, as is spent, where the velocity of each particle is less, but is continued thro' a less space. The dilemma, to which our author's reasoning seems to have reduced the doctrine of refrangibility, may therefore be consider'd as a probable argument for adopting this hypothesis of the propagation of light thro' an elastic medium.

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XXXIX. *The Case of the Operation for the Empyema, successfully performed by Joseph Warner, F. R. S. and Surgeon to Guy's Hospital.*

Read June 28, 1753. **O**N the 19 of March 1752, I did myself the honour of communicating to the Royal Society the case of John Hines, on whom I had performed the operation for the empyema



empyema with success. Since which time, I have performed the same operation a second time with equal benefit; and therefore presume to lay this case likewise before you, as a farther proof of its usefulness under the like circumstances.

**J**OHN Collier, aged 17 years, was admitted into Guy's Hospital on the 10 of May, 1753, on account of a complaint in his chest, which he had laboured under for three or four weeks. His symptoms were a continual pain in his left side, a difficulty in breathing, and an inability of lying on his right side, or of sitting upright, without greatly increasing his complaints. His pulse was quick, and low; he had a short cough, was a good deal emaciated, and appeared fallow in his complexion.

Upon examination, I perceived a small tumor, situated on the anterior part of the thorax obliquely, on the left side of the extremity of the sternum or breast-bone. There was not the least discoloration of the integuments. On pressing upon the tumor, his pain and difficulty of breathing were increased, and there appear'd something like a fluctuation under my fingers. He had never any rigor, which is a symptom generally attending the formation of matter; but from experience I have found, that the want of this symptom is no proof of the contrary,

From the foregoing circumstances, and symptoms, I made no doubt of the propriety of the operation, which I performed in the following manner:

The patient being properly situated and secured, I began with making an incision of about two inches long through the integuments, and tendinous ex-  
pansion

panfion of the oblique mufcles of the abdomen upon the moft prominent part of the tumor: then I proceeded to the making a fecond incifion, of an equal length with the former, tranfverfely thro' the upper part of the *rectus* mufcle (which had a perfect healthy appearance), directing my knife forwards, betwixt the cartilaginous portions of the feventh and eighth ribs, into the cavity of the thorax; upon which a thick clotted matter, to the quantity of 23 ounces and upwards, was difcharged. After the whole of the matter was difcharged, I introduced the fore-finger of my right-hand into the cavity, with which I evidently felt the lungs quite loofe and free from adhefion, the mediaftinum, and fuperior part of the diaphragm; which laft had been prefs'd fomewhat lower than its natural fituation by the weight of the incumbent matter. From whence it undoubtedly appear'd, that this great quantity of matter was contained in the cavity of the thorax.

After the whole of the matter was difcharged, I introduced a linen tent, properly fecured, into the cavity; which was continued to be introduced every day for about three weeks, now and then, as occafion required, making ufe of the prepared fponge-tent.

The difcharge of matter was confiderable for the firft week, when it began to decreafe gradually, till, at the end of three weeks, there was no difcharge at all.

From this time, fuperficial applications only were made ufe of. At the end of five weeks he was perfectly well, and has recovered his former plumpnefs, and healthy appearance.

I muft obferve to you, that, about two years ago, he received a violent blow on his left fide by a fall;

fall ; for which he had little or no care taken of him. He has ever since this accident had some complaints in his side at times, but not constantly ; nor have they ever been so bad, as to prevent his acting in his business as a sailor, till within a few weeks before he applied to me.

London, Hatton Garden,  
June 28, 1753.

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**XL.** *Extract of a Letter from Mr. James Dodson to Mr. William Mountaine, F.R.S.*

May 26, 1753.

Read July 5, 1753. **T**HE world has, without dispute, been obliged to the invention of fluxions, for many concise methods of calculating the peripheries, areas, and solidities, of curvilinear figures ; but it must be confessed, at the same time, that the most useful, even of those, had been computed before, tho' by methods more laborious ; and, consequently, since the truth of the principles of fluxions was long disputed, that art seems rather to have received, than to have afforded, any advantage, in those cases.

Neper and Briggs calculated their several tables of logarithms, with almost insuperable labour ; and Van Ceulen was rendered famous for his approximation to the quadrature of the circle, on account of the acknowledged tediousness of its computation. The methods of computing logarithms were indeed improved, by the assistance of the properties of the Hyperbola, and

the use of infinite series, before the great inventor of fluxions was known in the world: but then, the business of logarithms being purely arithmetical, the Hyperbola was foreign to the subject; and the nature of infinite series, tho' well adapted to the purpose, was at that time but little understood.

I am sometimes induced to believe, that, if the latter (*i. e.* the nature and use of infinite series) had arrived at any degree of perfection, before the invention of fluxions, most of the series, which are given for the above-named kind of calculations, and have been deduced from fluxional processes, would have been discovered without the assistance of them: and I am of this opinion, because I am certain, that many of them might have so been: to instance in both the cases above quoted; *viz.* the series, for making logarithms, and for rectifying the circle.

And first, the terms of one of the most simple series, for expressing the logarithm of a given number, is composed of the powers of the excess of that number, above unity, divided by their respective indices; of which the first, third, fifth, &c. terms are affirmative, and the second, fourth, sixth, &c. terms are negative; and the difference between the sums of the affirmative and the negative terms, is the Neperian, hyperbolic, or (as some call it) the natural logarithm of the given number.

Now a mathematician, who understands the nature and management of series (altho' wholly ignorant of fluxions, or what the justly celebrated Dr. Halley, in his admired investigation of this very series, published in N<sup>o</sup> 216 of the *Philosophical Transactions*, calls *ratuuncula*, &c.) might arrive at the same conclusion, in the following manner:

Since

Since the logarithm of unity is universally determined to be nothing; that of 2, 3, 4, 10, or any other number, consider'd as a root, is one; that of 4, 9, 16, 100, &c. consider'd as the square of that root, is two; and so on; it follows, that (in all cases) the logarithm of a greater number will exceed that of its lesser; and each logarithm will have some relation to the excess of its number, above unity, the number, whose logarithm is nothing: the terms of the series, therefore, which will represent the logarithm of any number, will consist of the powers of the excess of that number, above unity, with some, yet unknown, but constant coefficients.

That the logarithm of the square of any number is twice the logarithm of its root, is a well-known property of those artificial numbers; and therefore, the doubles of the particular terms of the assumed series will constitute a series, expressing the logarithm of the square of the given number.

But, by the fourth proposition of the second book of Euclid, the square of any quantity is equal the sum of the squares of its two parts, more a double rectangle of those parts; which, in this case (where the given number has been assumed, to consist of unity, and an excess) will be unity, more twice that excess, more the square thereof.

If, therefore, the several powers of the compound quantity (twice the excess of the given number above unity, more the square thereof) be multiplied by the above assumed coefficients, and afterwards ranged under each other, according to the powers of the said excess; their sums will again express the logarithm of the square of the given number.

Now, since the logarithm of the square of the given number may be thus expressed by two infinite series, each constituted of the excess thereof, above unity, and its powers; it follows, that the coefficients of the like powers of that excess, in each series, will be equal between themselves; and, consequently, the values of the unknown coefficients may be obtained, by simple equations; and these coefficients will, by the process annexed, appear to be, the reciprocals of the several indexes of the powers of that excess, affected alternately with the signs  $+$  and  $-$ , as was before found, by the quadrature of the Hyperbola, by Dr. Halley in the above-cited *Philosophical Transaction*, and by many who have used a fluxional process.

But, there is another logarithmic series equally simple with the former, consisting of the same terms, but all affirmative. This has been demonstrated to be the logarithm of that fraction, whose numerator is unity, and denominator a number, as much less than unity, as the former number exceeded it.

Now, if an infinite series be formed from that fraction, by actual division, it will consist of unity, and all the powers of that defect; and if the several powers of the excess of this infinite series above unity, be multiplied by the coefficients above-found, and ranged according to the powers of that defect, their sums will exhibit the above-described series for the logarithm of that fraction, as appears by the operation subjoin'd.

As to the application of these two series, and their sum, to the finding the logarithms of numbers; the same, being copiously treated of by Dr. Halley (in the *Philosophical Transaction* before quoted) there is

no occasion for the repetition thereof here. *Note*, The above tract of Dr. Halley's is printed with the explication of Sherwin's tables of logarithms (fol. 10) with many examples of the use thereof annexed.

Secondly, The terms of one of the best series, for the rectification of the circle, are composed of the odd powers of the tangent of any arc, not exceeding 45 degrees, severally divided by their respective indexes; of which the first, third, fifth, &c. terms are affirmative; and the second, fourth, sixth, &c. terms are negative; and the difference, between the sums of the affirmative and negative terms, is the length of that arc, of which the tangent, and its powers, constitute the series.

Now a mathematician, who understands the nature and management of series, altho' wholly ignorant of fluxions, might investigate this series in the following manner:

It has been geometrically demonstrated, that, the radius of a circle being unity, if the double of the tangent of any arc, be divided by the difference between unity, and the square of that tangent, the quotient will be the tangent of twice the arc.

Now if an infinite series be formed by actual division, its terms will consist of the doubles of the odd powers of the tangent, and will be all affirmative; which series will express the length of the tangent of the double of that arc, whose tangent and its powers constitute the same.

If a series, consisting of the tangent and its powers, with unknown coefficients, be assumed (as in the former case) to express the length of the arc; then the length of the double of that arc may be expressed two ways; *viz.* either by multiplying each term of

the series assumed by the number two ; or by finding the powers of the series above described (which exhibits the length of the tangent of the double arc) multiplying each power by its proper coefficient, ranging the products under each other (according to the powers of the tangent of the single arc) and finding their sum.

Now, since the length of the double arc may be thus expressed, by two infinite series, each constituted of the tangent of the single arc, and its powers ; therefore the coefficients of the like powers of that tangent, in each series, will be equal between themselves ; and consequently the values of the unknown coefficients may be obtained by simple equations.

Lastly, since the series, which gives the length of the tangent of the double arc consists only of the odd powers of the tangent of the single arc, therefore none of the even powers thereof can range therewith : now these will not occur in the odd powers of that series ; and therefore the series, assumed to express the length of the single arc, whose double is to be compared with the sum of the former, must consist only of the odd powers of that tangent ; and then the series first mentioned results from the operation, as will appear by examining the same, as hereto annexed.

I am thoroughly sensible, that, to the learned, who are already masters of the investigation of these series by other methods, this will appear to be an essay of more curiosity than use ; but, with regard to students, I humbly conceive it will have its advantages ; because by those, who are acquainted with the notation of algebra, and the manner of solving  
simple



simple equations, the nature of the series, and the operations above used, may be easily obtained; and they may be hereby enabled to make a scientific use of logarithms in arithmetical, and of the rectification of the circle, in geometrical calculations; which at present cannot be done, till a much greater progress is made in mathematical knowledge, without great labour; as well as to examine and correct the printed tables of both sorts, if dubious, or found to be erroneous.

The operation necessary to find the coefficients of a series, which will express the logarithm of a given number.

If the given number be represented by  $1+n$ , then the following series may be assumed to represent its logarithm:

$$n + xn^2 + yn^3 + zn^4 + un^5, \&c.$$

And  $2n + 2xn^2 + 2yn^3 + 2zn^4 + 2un^5, \&c.$  will represent the logarithm of the square of that number; viz. of  $1 + 2n + nn$ .

But, because  $2n + nn$  is the excess of  $1 + 2n + nn$ , above unity, therefore its logarithm will be also expressed by

$$2n + nn + x \times 2n + nn^2 + y \times 2n + nn^3 + z \times 2n + nn^4, \&c.$$

$$\begin{array}{lcl} \text{Now } \overline{2n + nn^2} & = & 4nn + 4n^3 + n^4 \\ \overline{2n + nn^3} & = & 8n^3 + 12n^4 + 6n^5, \&c. \\ \overline{2n + nn^4} & = & 16n^4 + 32n^5, \&c. \\ \overline{2n + nn^5} & = & 32n^5, \&c. \end{array}$$

Therefore,

$$\overline{2n+nn} = 2n+nn;$$

$$x \times \overline{2n+nn^2} = 4xnn + 4xn^3 + xn^4$$

$$y \times \overline{2n+nn^3} = 8yn^3 + 12yn^4 + 6yn^5 \&c.$$

$$z \times \overline{2n+nn^4} = 16zn^4 + 32zn^5 \&c.$$

$$u \times \overline{2n+nn^5} = 32un^5 \&c.$$

And the sum of these is equal to the logarithm of the square of  $1+n$ .

If an equation be formed, of the coefficients of  $n^3$ , in each of these expressions of the logarithm of that square,

Then  $2x = 1 + 4x$ ; whence  $-\frac{1}{2} = x$ .

And, by proceeding in the same manner with the coefficients of  $n^3$ ,  $n^4$ ,  $n^5$ , &c. and supplying the places of  $x$ ,  $y$ ,  $z$ , &c. as they arise, by the numbers (so found) we shall have

$$2y = -\frac{4}{2} + 8y; \quad \text{whence } +\frac{1}{3} = y;$$

$$2z = -\frac{1}{2} + \frac{12}{3} + 16z; \quad \text{whence } -\frac{1}{4} = z;$$

$$2u = \frac{6}{3} - \frac{32}{4} + 32u; \quad \text{whence } +\frac{1}{5} = u;$$

Consequently, the logarithm of  $1+n$  will be expressed by  $n - \frac{1}{2}n^2 + \frac{1}{3}n^3 - \frac{1}{4}n^4 + \frac{1}{5}n^5$ , &c. as above asserted.

Again, since  $\frac{1}{1-n} = 1+n+n^2+n^3+n^4+n^5$ , &c. as appears by the following division :

$$1-n \overline{) 1} \quad (1+n+n^2, \&c.$$

$$\begin{array}{r} 1-n \\ + n \\ + n - nn \\ \hline + nn \end{array}$$

And, since the excess of that series, above unity, is the series  $n + n^2 + n^3 + n^4$ , &c.

Therefore

Therefore the logarithm of  $\frac{1}{1-n}$  will consist of the sums of the powers of that series, multiplied by the above-found coefficients  $\frac{1}{1}, -\frac{1}{2}, +\frac{1}{3}, -\frac{1}{4}, +\frac{1}{5}$ , &c.

$$\text{Now the } \left\{ \begin{matrix} 2 \\ 3 \\ 4 \\ 5 \end{matrix} \right\} \text{ Power of that Series will be } \left\{ \begin{matrix} n^2 + 2n^3 + 3n^4 + 4n^5, \text{ \&c.} \\ n^3 + 3n^4 + 6n^5, \text{ \&c.} \\ n^4 + 4n^5, \text{ \&c.} \\ n^5, \text{ \&c.} \end{matrix} \right.$$

And,

$$\left\{ \begin{matrix} 1 \\ -\frac{1}{2} \\ +\frac{1}{3} \\ -\frac{1}{4} \\ +\frac{1}{5} \end{matrix} \right\} \text{ of that } \left\{ \begin{matrix} 1 \\ 2 \\ 3 \\ 4 \\ 5 \end{matrix} \right\} \text{ Power will be } \left\{ \begin{matrix} n + n^2 + n^3 + n^4 + n^5, \text{ \&c.} \\ -\frac{1}{2}n^2 - \frac{1}{2}n^3 - \frac{1}{2}n^4 - \frac{1}{2}n^5, \text{ \&c.} \\ +\frac{1}{3}n^3 + \frac{1}{3}n^4 + \frac{1}{3}n^5, \text{ \&c.} \\ -\frac{1}{4}n^4 - \frac{1}{4}n^5, \text{ \&c.} \\ +\frac{1}{5}n^5, \text{ \&c.} \end{matrix} \right.$$

The sums of which, viz.  $n - \frac{1}{2}n^2 + \frac{1}{3}n^3 - \frac{1}{4}n^4 + \frac{1}{5}n^5$ , &c. will be the logarithm of  $\frac{1}{1-n}$ , as above affirmed.

The operation necessary, to find the coefficients of a series, which will express the length of the arc of a circle, by the tangent of that arc, and its powers.

Let  $a$  represent the length of the arc, and  $t$  the length of the tangent thereof; then the tangent of that arc whose length is  $2a$ , will be  $\frac{2t}{1-t^2}$ ; which fraction is equal to the infinite series,  $2t + 2t^3 + 2t^5 + 2t^7 + 2t^9$ , &c. See the division.

$$\begin{array}{r}
 [ \ 282 \ ] \\
 1-tt) 2t \quad ( 2t + 2t^3 + 2t^5, \text{ \&c.} \\
 \underline{2t - 2t^3} \\
 \quad + 2t^3 \\
 \quad + 2t^3 - 2t^5 \\
 \quad \underline{\quad + 2t^5}
 \end{array}$$

And by performing the necessary multiplications, or divisions, it will also appear, that

$$\left[ \frac{2t}{1-tt} \right]^3 = 8t^3 + 24t^5 + 48t^7 + 80t^9, \text{ \&c.}$$

$$\left[ \frac{2t}{1-tt} \right]^5 = 32t^5 + 160t^7 + 480t^9, \text{ \&c.}$$

$$\left[ \frac{2t}{1-tt} \right]^7 = 128t^7 + 896t^9, \text{ \&c.}$$

$$\left[ \frac{2t}{1-tt} \right]^9 = 512t^9, \text{ \&c.}$$

Now if we assume, for the value of  $a$ , the following series,  $t + xt^3 + yt^5 + zt^7 + ut^9$ , \&c.

Then  $2t + 2xt^3 + 2yt^5 + 2zt^7 + 2ut^9$ , \&c.  $= 2a$ .

And because  $\frac{2t}{1-tt}$  is the tangent of the arc, whose length is  $2a$ , therefore

$$\left[ \frac{2t}{1-tt} + x \times \left[ \frac{2t}{1-tt} \right]^3 + y \times \left[ \frac{2t}{1-tt} \right]^5 + z \times \left[ \frac{2t}{1-tt} \right]^7 \right] \text{ \&c.} = 2a.$$

Which expression is equivalent to the sum of the following series; for

$2t$

$$\frac{2t}{1-tt} = 2t + 2t^3 + 2t^5 + 2t^7 + 2t^9, \&c.$$

$$x \times \left[ \frac{2t}{1-tt} \right]^3 = 8xt^3 + 24xt^5 + 48xt^7 + 80xt^9, \&c.$$

$$y \times \left[ \frac{2t}{1-tt} \right] = 32yt^5 + 160yt^7 + 480yt^9, \&c.$$

$$z \times \left[ \frac{2t}{1-tt} \right] = 128zt^7 + 896zt^9, \&c.$$

$$u \times \left[ \frac{2t}{1-tt} \right] = 512ut^9, \&c.$$

And (by making an equation, between  $2x$ , the coefficient of  $t^3$  in the first-found value of  $2a$ , and  $2+8x$ , the sum of the coefficients of  $t^3$  in the latter)  $2x = 2 + 8x$ ; whence  $-\frac{1}{3} = x$ .

And, by proceeding in the same manner, with the coefficients of  $t^5, t^7, t^9, \&c.$  and supplying the places of  $x, y, z, \&c.$  (as they arise) by the numbers (so found) we shall have

$$2y = 2 - \frac{24}{3} + 32y; \quad \text{whence } y = +\frac{1}{7}.$$

$$2z = 2 - \frac{48}{3} + \frac{160}{7} + 128z; \quad \text{whence } z = -\frac{1}{7}.$$

$$\text{And } 2u = 2 - \frac{80}{3} + \frac{480}{7} - \frac{896}{7} + 512u; \quad \text{whence } u = +\frac{1}{9}.$$

Therefore we may conclude, that  $t - \frac{1}{3}t^3 + \frac{1}{7}t^5 - \frac{1}{7}t^7 + \frac{1}{9}t^9, \&c. = a$ .

The application of this series to the rectification of the circle is extant in many authors; particularly in Sherwin's tables of logarithms, above-quoted, folio 55.

When the arc is just 45 degrees, then  $t = 1$ , and the series becomes  $\frac{1}{1} - \frac{1}{3} + \frac{1}{7} - \frac{1}{7} + \frac{1}{9}, \&c.$  which converges exceedingly slow; but, by the assistance of a

N n 2

method,

method, given in the appendix to M. De Moivre's *Miscellanea Analytica*, it may be transformed to another, converging quicker; which method is applied to this very series, in folio 362 of the *Mathematical Repository*, Vol. I.

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XLI. *A Letter from John Lining, M. D. of Charles-Town, South-Carolina, to the Rev. Thomas Birch, D. D. Secr. R. S. concerning the Quantity of Rain fallen there from January 1738, to December 1752.*

Rev. Sir,

South Carolina, Charles-Town, April  
9, 1753.

Read July 8,  
1753.

**T**HE favourable reception, which my former papers met with from the Royal Society, encourages me to send you a table of the quantity of rain, which fell in Charles-Town for these 15 years last past; which, if continued for half a century, might be of use, in discovering to us the changes made in a climate, by clearing the land of its woods. Tho' I formerly sent a table of the rain from 1738 to 1745 inclusive, which is publish'd in N<sup>o</sup> 487 of the *Philosophical Transactions*; yet, as I thought it would be more convenient to bring the whole into one view, I have not only added to this table the rain of those years, but have likewise distinguished the quantity which fell in the several seasons. In this table I continued the old stile to the first of last February, that the mean quantity, in each month, and in the different seasons, might be given exactly.

As



A TABLE of the Depth of Rain, in Inches and millifimal Parts, which fell in Charles-Town.

	1738	1739	1740	1741	1742	1743	1744	1745	1746	1747	1748	1749	1750	1751	1752	Greatest	Least	The Means
January	1.097	2.310	4.875	4.492	2.189	3.172	1.994	0.863	1.144	3.429	2.112	1.056	2.563	None	3.597	4.873		2.326
February	4.416	2.875	3.084	4.615	1.650	2.435	3.063	7.739	2.701	2.860	1.573	4.516	3.135	5.374	3.798	7.739	0.798	3.385
March	4.532	5.609	1.141	5.713	5.203	0.621	0.582	3.229	1.628	2.585	3.047	7.475	0.943	1.342	1.716	7.475	0.621	3.024
April	1.082	0.195	1.092	1.308	0.918	5.292	2.866	3.842	1.128	0.292	0.979	1.760	2.310	2.310	0.440	5.292	0.195	1.721
May	3.127	5.120	5.612	4.841	5.898	2.535	2.871	1.832	3.988	0.924	1.826	5.555	2.371	5.533	2.794	5.898	1.826	3.655
June	1.567	5.839	4.648	5.538	3.250	1.903	5.814	9.510	4.109	2.470	1.859	4.686	8.690	2.462	2.618	15.839	1.567	5.000
July	10.660	5.452	3.013	3.399	1.252	7.738	8.437	6.771	9.895	6.413	9.273	6.219	5.687	6.544	1.485	10.660	1.252	6.149
August	4.104	12.211	7.301	7.144	7.647	3.767	4.202	9.339	6.114	4.895	6.881	11.124	5.346	12.144	10.725	12.211	3.767	7.530
September	10.792	4.834	3.200	6.734	2.895	4.686	5.657	0.754	0.932	7.216	7.442	1.298	12.370	11.671	14.663	14.663	0.754	6.342
October	1.358	6.593	1.358	3.399	0.759	1.672	1.595	2.962	0.506	9.504	5.550	3.900	5.000	0.352	1.199	9.504	0.352	3.040
November	2.656	1.235	1.848	2.964	3.388	3.220	1.562	0.682	3.586	1.056	5.368	1.238	3.137	0.682	0.814	5.368	0.682	2.225
December	3.877	3.689	2.736	1.919	0.957	2.706	9.680	2.623	3.916	2.921	5.588	5.594	4.609	2.409	2.035	9.680	0.957	3.684
Spring	10.030	8.679	5.317	11.636	7.771	8.348	6.511	14.810	5.457	5.737	5.599	13.751	6.388	9.026	2.954	14.810	2.954	8.068
Summer	15.354	26.411	13.273	13.778	10.400	12.176	17.122	18.113	17.992	9.807	12.958	16.460	16.748	14.569	6.897	26.411	6.897	14.804
Autumn	16.254	23.638	11.759	17.277	11.301	10.125	11.454	13.055	7.552	21.615	19.873	16.322	22.716	24.167	26.587	26.587	7.552	16.913
Winter	8.843	9.797	9.076	7.072	7.517	7.920	12.105	4.449	10.931	6.089	12.012	9.355	7.744	6.688	5.456	12.105	4.449	8.340
Total Depth	49.926	65.962	65.962	52.086	36.006	59.747	48.323	50.146	39.653	44.565	51.498	54.421	56.159	50.853	42.884	65.962	36.006	48.023

The Depth of Rain in January 1753. O. S. was 2.607 inches.

Place this between p. 284, 285.







As we have many thunder-gusts in the hot months, in which a vast quantity of rain falls, the depth of the rain, in these months, is thereby greatly increased; for then we have very little rain, excepting in thunder-showers.

On the 30 of June, 1750, in a thunder-storm, there fell, in two hours, 5.335 inches of rain.

On the 16 of September, 1751, there fell, in 24 hours (but the greatest part in 6 hours) 9.955 inches of rain.

On the 15 of September, 1752, during the time of the most violent hurricane, that was ever felt in this town, the depth of rain, which fell, was only 3.740 inches, and the greatest part of that was the spray of the sea.

From the 17 Sept. 1751, to the 7 August 1752, was the driest season ever known in this province.

Since I sent an abstract of my meteorological tables to the Royal Society, I have seen Fahrenheit's thermometer in the shade once down at the 10 degree; and once last summer it rose to the 100 degree. I am,

S I R,

Your most humble servant,

John Lining.

XLII. *A Letter from Mr. Emanuel Mendez da Costa, F. R. S. to the Rev. Thomas Birch, D. D. Secr. R. S. concerning the Fossil, found at Dudley in Staffordshire, and described in the Phil. Transf. N. 496.*

S I R,

Read July 12, 1753. **T**HE famous fossil, which Dr. Lyttelton shew'd to the Royal Society some time ago from Dudley in Staffordshire, and which is engraved and described in N<sup>o</sup> 496 of the *Transactions*, caused many arguments to what class of animals it belonged. Dr. Pococke afterwards produced two or three specimens of it extended, which proved it of the crustaceous tribe of animals. But none of his said specimens being very fair, I have taken the liberty to send you a very fair specimen of the said fossil extended, sent me last week from the iron mines at Colnbrookdale in Shropshire, and which absolutely determines me to pronounce it to be the remains of a crustaceous animal, of that kind called *Pediculi marini*, which are scaled all round, and can at will roll themselves up: And this particular kind (not to my knowlege yet discover'd from sea) may be justly synonymed *Pediculus marinus major trilobos*.

Tho' I heretofore thought it not described by any English author, yet I find it is described and figured, tho' badly, by our late member Mr. Edw. Lhuyd, in his *Lithophylacium Britannicum Ichnographia*, Epist. 1. p. 96. Table XXII. who found them in  
plenty

plenty in quarries, *juxta ædes nob. v. D. Gryfdii Rice de Newton, arm. prope oppidum Sancti Teilavii, in comitatu Mariduniæ.* He calls it *Buglossa curta strigosa*. He also gives the figure of it without any description, in the *Phil. Transf.* N<sup>o</sup> 243.

I beg you would communicate this supplement to the Royal Society in my name. I remain, with great esteem,

S I R,

London, July 12,  
1753.

Your very humble servant,

Emanuel Mendez da Costa.

**XLIII.** *Letters relating to a Theorem of Mr. Euler, of the Royal Academy of Sciences at Berlin, and F. R. S. for correcting the Aberrations in the Object-Glasses of refracting Telescopes.*

I.

*A Letter from Mr. James Short, F. R. S. to Peter Daval, Esq; F. R. S.*

Dear Sir,

Read April 9, 1752. **T**HERE is published, in the Memoirs of the Royal Academy at Berlin, for the year 1747, a theorem by Mr. Euler, in which he shews a method of making object-glasses of telescopes, in such a manner, as not to be affected  
by

by the aberrations arising from the different refrangibility of the rays of light; these object-glasses consisting of two *meniscus* lens's, with water between them.

Mr. John Dollond, who is an excellent analyst and optician, has examined the said theorem, and has discovered a mistake in it, which arises by assuming an hypothesis contrary to the established principles of optics; and, in consequence of this, Mr. Dollond has sent me the inclosed letter, which contains the discovery of the said mistake, and a demonstration of it.

In order to act in the most candid manner with Mr. Euler, I have proposed to Mr. Dollond to write to him, shewing him the mistake, and desiring to know his reasons for that hypothesis; and therefore I desire, that this letter of Mr. Dollond's to me may be kept amongst the Society's papers, till Mr. Euler has had a sufficient time to answer Mr. Dollond's letter to him. I am,

S I R,

Surrey-street, April 9,  
1752.

Your most humble servant,

James Short.

*A Letter from Mr. John Dollond to James Short, A. M. F. R. S. concerning a Mistake in M. Euler's Theorem for correcting the Aberrations in the Object-Glasses of refracting Telescopes.*

S I R,

Read Nov. 23, 1752. **T**HE famous experiments of the prism, first tried by Sir Isaac Newton, sufficiently convinced that great man, that the perfection of telescopes was impeded by the different refrangibility of the rays of light, and not by the spherical figure of the glasses, as the common notion had been till that time; which put the philosopher upon grinding concave metals, in order to come at that by reflexion, which he despair'd of obtaining by refraction. For, that he was satisfied of the impossibility of correcting that aberration by a multiplicity of refractions, appears by his own words, in his treatise of Light and Colours, *Book I. Part 2. Prop. 3.* " I  
 " found moreover, that when light goes out of air  
 " through several contiguous mediums, as through  
 " water and glass, as often as by contrary refractions  
 " it is so corrected, that it emergeth in lines parallel  
 " to those in which it was incident, continues ever  
 " after to be white. But if the emergent rays be in-  
 " clined to the incident, the whiteness of the emerging  
 " light will by degrees, in passing on from the place  
 " of emergence, become tinged in its edges with co-  
 " lours."

It is therefore, Sir, somewhat strange, that any body now-a-days should attempt to do that, which

so long ago has been demonstrated impossible. But, as so great a mathematician as Mr. Euler has lately published a theorem \* for making object-glasses, that should be free from the aberration arising from the different refrangibility of light, the subject deserves a particular consideration. I have therefore carefully examined every step of his algebraic reasoning, which I have found strictly true in every part. But a certain hypothesis in page 285. appears to be destitute of support either from reason or experiment, though it be there laid down as the foundation of the whole fabrick. This gentleman puts  $m:1$  for the ratio of refraction out of air into glass of the mean refrangible rays, and  $M:1$  for that of the least refrangible. Also for the ratio of refraction out of air into water of the mean refrangible rays he puts  $n:1$ , and for the least refrangible  $N:1$ . As to the numbers, he makes  $m = \frac{3}{2} \frac{1}{5}$ ,  $M = \frac{7}{5} \frac{7}{5}$ , and  $n = \frac{4}{3}$ ; which so far answer well enough to experiments. But the difficulty consists in finding the value of  $N$  in a true proportion to the rest.

Here the author introduces the supposition above-mention'd; which is, that  $m$  is the same power of  $M$ , as  $n$  is of  $N$ ; and therefore puts  $n = m^1$ , and  $N = M^a$ . Whereas, by all the experiments that have hitherto been made, the proportion will come out thus,  $m-1 : n-1 :: m-M : n-N$ .

The letters fixed upon by Mr. Euler to represent the radii of the four refracting surfaces of his compound object-glass, are  $f$   $g$   $b$  and  $k$ , and the distance of the object he expresses by  $a$ ; then will the focal distance

$$bc = \frac{1}{n \left( \frac{1}{f} - \frac{1}{b} \right) + m \left( \frac{1}{f} - \frac{1}{g} + \frac{1}{b} - \frac{1}{k} \right) - \frac{1}{a} - \frac{1}{f} + \frac{1}{k}} \quad \text{Now,}$$

says

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\* Vide Memoires of the Royal Academy of Berlin for the Year 1747.



says he, it is evident, that the different refrangibility of the rays would make no alteration, either in the place of the image, or in its magnitude, if it were possible to determine the radii of the four surfaces, so as to have  $n(\frac{1}{g} - \frac{1}{b}) + m(\frac{1}{f} - \frac{1}{g} + \frac{1}{k} - \frac{1}{b}) = N(\frac{1}{g} - \frac{1}{b}) + M(\frac{1}{f} - \frac{1}{g} + \frac{1}{n} - \frac{1}{b})$ . And this, Sir, I shall readily grant. But when the surfaces are thus proportioned, the sum of the refractions will be  $= 0$ ; that is to say, the emergent rays will be parallel to the incident. For, if  $n(\frac{1}{g} - \frac{1}{b}) + m(\frac{1}{f} - \frac{1}{g} + \frac{1}{b} - \frac{1}{k}) = N(\frac{1}{g} - \frac{1}{b}) + M(\frac{1}{f} - \frac{1}{g} + \frac{1}{b} - \frac{1}{k})$ , then  $n - N(\frac{1}{g} - \frac{1}{b}) + m - M(\frac{1}{f} - \frac{1}{g} + \frac{1}{b} - \frac{1}{k}) = 0$ . Also if  $n - N : m - M :: n - 1 : m - 1$ , then  $n - 1(\frac{1}{g} - \frac{1}{b}) + m - 1(\frac{1}{f} - \frac{1}{g} + \frac{1}{b} - \frac{1}{k}) = 0$ ; or otherwise  $n(\frac{1}{g} - \frac{1}{b}) + m(\frac{1}{f} - \frac{1}{g} + \frac{1}{b} - \frac{1}{k}) - \frac{1}{f} + \frac{1}{k} = 0$ ; which reduces the denominator of the fraction expressing the focal distance to  $\frac{1}{a}$ . Whence the focal distance will be  $= a$ ; or, in other words, the image will be the object itself. And as, in this case, there will be no refraction, it will be easy to conceive how there should be no aberration.

And now, Sir, I think I have demonstrated, that Mr. Euler's theorem is intirely founded upon a new law of refraction of his own; but that, according to the laws discover'd by experiment, the aberration arising from the different refrangibility of light at the object-glass cannot be corrected by any number of refractions whatsoever. I am,

S I R,

London, March 11, Your most obedient humble servant,  
1752.

John Dollond.

*Mr. Euler's Letter to Mr. James Short,  
F. R. S.*

Monfieur,

Read July 8,  
1753.

**V**OUS m'avez fait un tres fenfible plaisir, en ayant difpofé M. Dollond de remettre la propofition de fes objections contre mes verres objectifs, jufqu' à ce que j'y aurois repondu, et je vous en fuis infiniment obligé. Je prend donc la liberté de vous adrefler ma reponfe à lui, en vous priant, après l'avoir daignée de votre examen, de la vouloir bien lui remettre : et en cas que vous jugiez cette matiere digne de l'attention de la Societé Royale, je vous prierois de lui communiquer les preuves detaillées de ma theorie, que j'ai expofée dans cette lettre. Cependant j'efpere, que M. Dollond en fera fatisfait, puifque je tombe d'accord avec lui du peu de fucces, qu'on fauroit fe promettre de mes objectifs, en les travaillant felon la maniere ordinaire.

J'ai l'honneur d'etre, avec la plus parfaite confideration,

Monfieur,

Berlin, 19 Juin,  
1752

Votre tres humble, et

tres obéiffant ferviteur,

L. Euler.

*A Monsieur Monsieur Dollond.*

Monsieur,

Read July 8,  
1753.

**E**TANT très sensible à l'honneur que vous me faites, au sujet des verres objectifs, que j'avois proposé, j'ai celui de vous marquer d'abord ingenuement, que j'ai rencontré aussi ici le plus grands obstacles dans l'exécution de ce dessein, vu qu'il s'agit de quatre faces, qui doivent être travaillées exactement selon les proportions que j'avois trouvées ; cependant ayant fait les expériences sur quelquesuns, qui parurent le mieux réussir, nous avons trouvé, que l'intervalle entre les deux foyers des rayons rouges et violets étoit beaucoup plus petit, qu'il ne seroit d'un verre simple de la même distance focale. Neantmoins je dois avouer, qu'un tel verre, quand même il bien seroit parfaitement exécuté sur mes principes, auroit d'autres défauts, qui le mettroient au dessous même des verres ordinaires : c'est qu'un tel verre n'admet qu'un très petite ouverture en conséquence des grandes courbures, qu'on doit donner aux faces intérieures : de sorte que lorsqu'on donne une ouverture ordinaire, l'image devient très confus.

Ainsi puisque vous vous êtes donné la peine, Monsieur, d'exécuter de tels verres, en en faisant des expériences \*, je vous prie de bien distinguer les défauts, qui peuvent naître de la diverse réfrangibilité des rayons, de ceux, qui viennent d'une trop grande ouverture : pour cet effet vous n'aurez qu'à laisser une très petite ouverture.

Or

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\* Mr. Dollond, in his letter to Mr. Euler, here referred to, does not say that he had made any trials himself, but only he had understood that such had been made by others, without success.

Or si ma theorie etoit juste, dont j'aurai bientot l'honneur de parler, il feroit moyen de remedier à ce defaut; il faudroit renoncer à la figure spherique qu'on donne ordinairement aux faces des verres, et tacher de leur donner une autre figure, et j'ai remarqué que la figure d'une parabole leur procureroit l'avantage, qu'ils admettroient une ouverture tres considerable. Notre savant M. Lieberkuhn s'est appliqué à travailler des verres dont la courbure des faces décroît depuis le milieu vers le bords, et il s'en est aperçu de tres grands avantages. Par ces raisons je crois, que ma theorie ne souffre encore rien de ce coté.

Pour la theorie, je conviens avec vous, monsieur, que posant la raport de refraction d'un milieu dans un autre quelconque pour les rayons moyens comme  $m$  à 1, et pour les rayons rouges comme  $M$  à 1, la raison de  $m-M$  à  $m-1$  sera toujours si à peu près constant, qu'elle satisfera à toutes les experiences, comme la grand Newton a remarqué. Cette raison ne differe non plus de ma theorie que presque imperceptiblement: car puisque je soutiens que  $M=m^a$ , et que  $m$  differe ordinairement fort peu de l'unité, soit  $m=1+\omega$ ; et puisque  $M=m^a=1+\alpha l m$  à peu pres, et  $l(1+\omega)=lm=\omega$ , aussi fort à peu pres, j'aurai  $m-M=1+\omega-1-\alpha\omega=(1-\alpha)\omega$ , et  $m-1=\omega$ , donc la raison  $\frac{m-M}{m-1}$  sera  $=1-\alpha$ , ou fort à peu pres constante. Delà je conclud, que les experiences d'ou le grand Newton a tiré son raport, ne sauroient etre contraires à ma theorie.

En second lieu, je conviens aussi que si la raison  $\frac{m-M}{m-1} = \text{Const.}$  etoit juste à la rigueur, il n'y auroit plus

plus moyen de remedier au defect qui resulte de la diverse refrangibilité des rayons, de quelque maniere qu'on disposeroit divers milieux transparens, et que l'intervalle entre les divers foyers tiendrait toujours un raport constant à la distance focale entiere du verre. Mais c'est precisement cette consideration, qui me fournit le plus fort argument : l'oeil me paroît une telle machine dioptrique parfaite, qui ne se ressent en aucune maniere de la diverse refrangibilité des rayons : quelque petite que soit sa distance focale, la sensibilité est si grande, que les divers foyers, s'il y en avoit, ne manqueroient pas de troubler tres considerablement la vision. Or il est bien certain, qu'un oeil bien constitué ne sent point l'effet de la diverse refrangibilité.

La structure merveilleux de l'oeil, et les diverses humeurs, dont il est composé, me confirme infiniment dans ce sentiment. Car s'il s'agissoit seulement de produire une representation sur le fond de l'oeil, une seule humeur auroit été suffisante ; et le Createur n'y auroit pas seulement employé plusieurs. Delà je conclud, qu'il est possible d'anéantir l'effet de la diverse refrangibilité des rayons par une juste arrangement de plusieurs milieux transparens, donc puisque cela ne seroit pas possible, si la formule  $\frac{m - M}{m - 1} =$

Const. étoit vraie à la rigueur, j'en tire la consequence qu'elle n'est pas parfaitement conforme à la nature.

Mais voila une preuve directe de ma these : je conçois diverse milieux transparens, *A, B, C, D, E, etc.* qui different entr'eux également par raport à leur densité optique : desorte que la raison de refraction de chacun dans le suivant soit le meme. Soit donc dans le  
passage

passage du premier dans le second la raison de refraction pour les rayons rouges  $= r : 1$ , et pour les violets  $= v : 1$ ; qui sera la même dans le passage du second dans le troisième, de celui-ci dans le quatrième, du quatrième dans le cinquième, et ainsi de suite. Delà il est clair, que dans le passage du premier dans le troisième sera  $= r^2 : 1$  pour les rayons rouges, et  $= v^2 : 1$  pour les violets: de même dans le passage du premier dans le quatrième les raisons seront  $r^3 : 1$  et  $v^3 : 1$ .

Donc si dans le passage dans un milieu quelconque la raison de refraction des rayons rouges est  $= r^n : 1$ , celle des rayons violets sera  $= v^n : 1$ ; tout cela est parfaitement conforme aux principes du grand Newton. Posons  $r^n = R$ , et  $v^n = V$ , de sorte que  $R : 1$ , et  $V : 1$  expriment les raisons de refraction des rayons rouges et violets dans un passage quelconque: et ayant  $n l r = l R$  et  $n l v = l V$  nous aurons  $l R : l r = l V : l v$ , ou  $\frac{l R}{l V} = \frac{l r}{l v}$ . Ou bien mettes  $v = r^a$ , et à cause

de  $l v = a l r$ , on aura  $\frac{l R}{l V} = \frac{1}{a}$ , ou  $l V = a l R$ , et partant  $V = R^a$ .

Voilà donc le fondement du principe, que j'ai employé dans ma pièce, qui me paroît encore inébranlable: cependant j'en soumetts la décision à l'illustre Société Royale, et à votre jugement en particulier, ayant l'honneur d'être avec la plus parfaite considération, Monsieur,

Berlin, Juin 15,  
1752.

Votre très humble  
et très obéissant serviteur,

L. Euler.  
XLIV.

XLIV. *A remarkable Case of Fragility, Flexibility, and Diffolution, of the Bones ; communicated by John Pringle, M.D. F.R.S.*

Read July 12, 1753. **M**ARY Hayes, of Stoke-Holy-Cross, near Norwich in Norfolk, gave the following Account, June 21, 1752 :

That she was born Jan. 11, 1718, and never married, or was addicted to any kind of Intemperance : That her father was unhealthy a great part of his life, but she knew not what disease he was subject to : That her mother died when she was a child ; but she did not remember she ever heard of her being unhealthy : That she herself was always look'd upon as an healthy strong girl, till about 15 years of age ; then fell into the green-sickness, and took various medicines, to no purpose : That this disease, as far as she could recollect, was all she had to complain of ; doing the ordinary work in a farmer's house, till October 1748 : Then was seiz'd with pain universally, attended with feverish symptoms. Thus she continued some weeks ; after which the pain was chiefly confined to her thighs and legs, but not increased by external pressure : That, in September 1749, she broke her leg, as she was walking from the bed to her chair, without falling down, and heard the bones snap. The fracture was properly treated, and regard had to her indisposition ; but no callus was generated ; the bones growing flexible from the knee to the ankle in a few months ; as did those of her other leg. Soon after, those of her thighs were visibly affected in the like manner.

Both legs and thighs then became very œdematous, and subject to excoriate, discharging a thin yellow ichor.

The winter after breaking her leg, she had symptoms of the scurvy, and bled much at the gums.

Many eminent physicians, who were of opinion, that this disease of the bones might arise from acidity abounding in the blood, prescribed for her, but without effect: unless the regularity of her menstruation for the last eighteen months may be attributed to a chalybeate medicine: tho' medicines of that nature had no such effect formerly, when she was in a condition to take exercise, and regularly persisted in the use of them.

For some considerable time past she had found little alteration in her complaints in general; thought her appetite and digestion rather better, but that the difficulty of breathing, which she had long labour'd under, gradually increased: and the thorax appeared so much straiten'd, as necessarily impeded the expansion of the lungs. Her spine became much distorted: any motion of the vertebræ of her loins gave extreme pain; and her thighs and legs were become intirely useless; which wholly confined her to her bed, in a sitting posture: and the bones she rested upon, having lost their solidity, were much spread. Also the ends of her fingers and thumbs, by frequent endeavours to lift herself up for ease, became very broad and flat. Then she measured but four feet; tho', before this disease came upon her, she was about five feet and a half high, and well-shaped.

This is the best information that could be obtained from her own mouth, and what was observed in the  
case



case before, and at the first-mention'd time, when she readily consented to the examination of her body, &c. after death.

From that time to her death, which happened Feb. 6, 1753, the chief thing she complain'd of, and what the people about her observed, was a gradual increase of difficulty of breathing; a wasting of her flesh; a cessation of her menstruation for the last four months; a tendency in her legs to mortify, which had long been anasarcaous, and excoriated almost all over; she retaining her senses perfectly to the last moment of her life, and dying without shewing the least signs of the agonies of death.

Two days after death, her limbs being first well stretched out, she was exactly measured, and found wanting of her natural stature more than two feet two inches. Then the thorax and abdomen were opened, the sternum being intirely removed, with part of the ribs, in order to gain at once a full view of those cavities, and discover how the viscera therein contained had obstructed each other in their respective functions. The heart and lungs were found, but flaccid, and much confined in their motion; to which the enormous size of the liver contributed in some measure, extending quite cross the abdomen, and bearing hard against the diaphragm. The lungs did not adhere to the pleura; nor was the liver scirrhou, but faulty only in its bulk. The mesentery was found, except only one large scirrhou gland upon it. The spleen extremely small. Nothing else was found observable in those cavities.

The skull was not opened, to examine the brain, as intended, we wanting time; the minister waiting at

church for interment, and the relations growing impatient; but we had no reason to suspect any defect there, from any previous complaint.

All her bones were more or less affected, and scarce any would resist the knife: those of the head, thorax, spine, and pelvis, nearly to the same degree of softness: those of the lower extremities much more dissolved than those of the upper, or of any other part. They were cut quite thro' their whole length, without turning the edge of the knife, and much less resistance found, than firm muscular flesh would have made; being changed into a kind of parenchymous substance, like soft dark coloured liver, only meeting here and there with bony *laminae*, thin as an egg-shell.

Those bones were most dissolved, which, in their natural state, were most compact, and contained most marrow in their cavities; and the heads of them were least dissolved.

This, perhaps, is the more worthy observation, as it held good throughout, and looks as if the wonderful change they had undergone might be caused by the marrow having acquired a dissolving quality: for it was evident the dissolution began within, from the bony *laminae* remaining here and there on the outside, and no-where else, and the pain not being increased at first by external pressure.

The periosteum was thicker than ordinary: the cartilages rather thinner; but no-where in a state of dissolution like the bones.

The day after this examination, some of the whole substance of the leg and thigh-bones, that was intirely dissolved into a kind of pulp, was sent to an ingenious chemist;

chemist; and, by the experiments which he made, he said he could discover neither acid nor alkali prevailing in it.

We, whose names are subscribed, do attest the truth of this relation.

June 25, 1753.

B. DACK, Physician.

EDWARD COOPER, } Surgeons.  
B. GOOCH, }

XLV. *Astronomical Observations made in*  
*Surry-street, London, by J. Bevis, M. D.*  
*and James Short. A. M. F. R. S.*

Read Nov. 8,  
1753.

Eclipse of *Venus* by the *Moon*.

Apparent Time.

d h ' "

1753 July 26 16 2 17 Venus totally hid by the Moon.

17 5 6 Her northern cusp emerg'd:  
 and, a few seconds after,  
 her southern one.

5 31 Venus was totally emerged.

All these with a reflector  
 of two feet focus.

Then her diameter was  
 found to be  $32\frac{3}{4}''$ , with  
 a new kind of micrometer  
 (of which more hereafter); and also with  
 one of Mr. Graham's  
 fort,

fort, in a two-feet Gregorian reflector.

	d	h	'	"	
July 26	21	4	18		The Moon's consequent limb passed the meridian.

Eclipse of *Mars* by the *Moon*

Aug. 20	17	6	49½		The Moon's consequent limb passed the meridian.
	8	4			Mars's centre passed the merid.
					His diameter then, with both micrometers, 13½".
					The Moon's diameter 31' 21".
18	6	59½			Mars totally hid by the Moon with a reflector of four feet focus.
					The emerfion could not be seen for clouds.

Occultation of  $\beta$  *Capricorni* by the *Moon*.

Oct. 5	7	16	50		The Moon's preceding limb passed the meridian.
	20	4			A small star, which preceded $\beta$ , passed the meridian.
	20	19			$\beta$ passed the meridian.
					Presently after, the Moon's diameter was found to be 29' 48", with the new micrometer, applied to a reflector of two feet focus.
8	21	3			The small star eclips'd by the Moon.
	28	48			$\beta$ eclipsed by the Moon.

Oct.

- d h ' "
- Octob. 5 9 48 24  $\beta$  emerged from the Moon.  
 [The three last with a two-foot reflector.]
- 48 44 $\frac{1}{2}$   $\beta$  passed the horary wire of the micrometer in the equatorial instrument of two feet focus.
- 48 55 The Moon's preceding limb passed the same; the star then apparently north of the Moon's south limb 15' 4" in declination.

### Eclipse of the Sun.

- Octob. 25 20 30 10 The eclipse had been some time begun; but, for clouds, could not be seen till now; when the distance between the cusps, measur'd with the new micrometer, applied to a two-foot reflector, was 12' 26 $\frac{1}{2}$ ".
- 21 15 23 The distance between the cusps 29' 49".
- 18 6 The distance between the visible limbs of the Sun and Moon 11' 32".
- 22 18 56 The distance between the cusps 24' 12 $\frac{1}{2}$ ".

The day before, about 10 in the morning, the Sun's horizontal diameter was 32' 17".

These measures were all taken, when the sun continued visible but for a few seconds, through the  
 interstices

interstices of flying clouds; and yet, from the nature of this micrometer, they may be very safely relied upon: though it would have been impossible to have catch'd any one of them with the common micrometer.

The principle on which this most excellent instrument is constructed, was laid before this Society last May: and it is to be hoped, that Mr. Dollond will evince the certainty of its measurements, from the least to the greatest angle it is capable of comprehending; and that, under every consideration of reflexion as well as refraction by spherical surfaces; so as to leave no room for such objections or cavils, as otherwise may probably be brought against it. For our own parts, we are fully satisfied of the justness thereof, from a great variety of trials and comparisons. That which we have hitherto used, is the first that has been made of the kind; and might, perhaps, have been better constructed in some respects, tho' in nothing material.

Applied to a reflector of only two feet, the scale is as large as the common micrometer can have in a forty-foot refractor; and all is done without the help of screws or wires; so that there is no need of illuminating.

In virtue of such a scale it is, that even fractions of seconds may be depended upon; as we have found, by often repeated trials on the diameters of the planets. These, as well as small distances of stars, may be measured in all directions, with equal and almost incredible facility, without a polar axis; as well out of doors, in a rough wind, as within.

More

More of its advantages might be mentioned, but these, we apprehend, may be enough to recommend it at present.

J. Bevis.  
Ja. Short.

XLVI. *A Letter from Mr. John Ellis to Mr. Peter Collinson, F. R. S concerning a Cluster-Polype, found in the Sea near the Coast of Greenland.*

S I R,

Read Nov. 8, 1753. **T**HE marine production, that you were so obliging to send me, appears to be an animal, not a vegetable, as your friend call'd it, who sent it to you. Upon examining it, I find it to be a species of cluster-polype, consisting of many bodies united at one common base. This specimen appears to have three-and-twenty distinct ones. I have since seen another, that was taken at the same time, that had between thirty and forty.

Each body is furnished at the top with eight arms or *tentaculi*, which expand themselves in the form of a star. Each arm is again furnished on each side with a row of small fibres, which seem to do the office of fingers. In the centre of the eight arms appears the mouth, surrounded by six little semicircular lips standing upright.

Upon dissecting one of the bodies lengthwise, it appeared to consist of a strong muscle, contracted

Qq into

into little waves or wrinkles. In the little cavities of these I observed sundry small seed-like particles, perhaps the spawn of the animal: when magnified, they appeared of a spherical form, a little compressed.

To the centre of the base, where the cluster of polypes unite, and make one body, there grows a four-square bony stem of six feet long, having four grooves, one on each side. At the joining to the fleshy part, the bony stem is very small, and a little twisted, like the turn of a screw, extending a membrane like a bladder, for about two or three inches in length, and near an inch in breadth, from the fleshy part downwards. The membrane then begins to close insensibly, and becomes a cuticular covering to the bony stem, which now increases gradually, till it becomes a quarter of an inch square. Within five or six inches of the bottom of the stem the bony part begins to grow smaller, till it comes to a point; and the cuticular part becomes cartilaginous, and supplies this tapering part with a quantity of this elastic substance, equal to the deficiency of the bone.

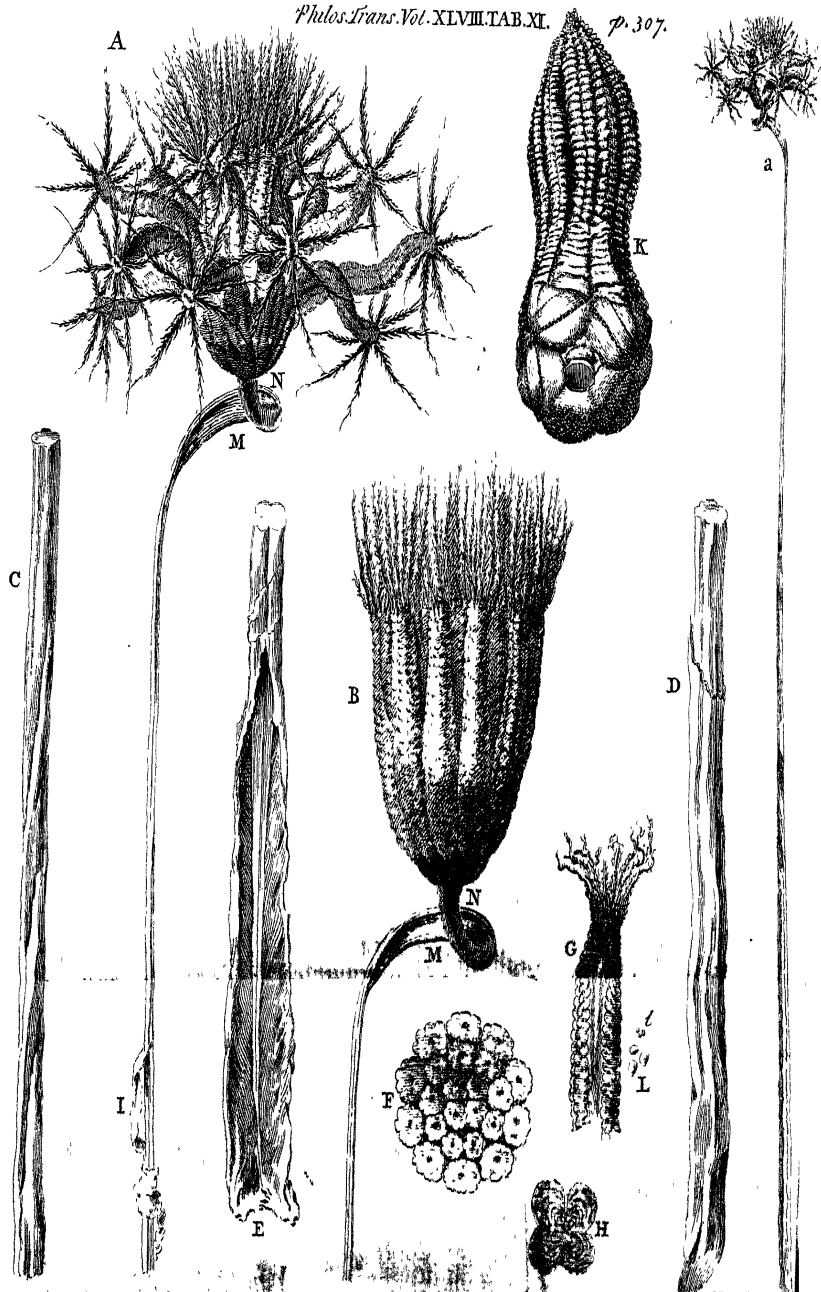
The use of this membrane, or bladder-like, skin at the top of the stem, may possibly be intended to give the animal a power to raise and fall itself in the water at pleasure.

It appears from the twist in one part of the stem, that the stem, when very small, and not so bony, had met with some violence, that had turned it out of its direction; the mark of which has still grown on with it: for the stem of the other specimen, taken at the same time, was quite even.

Upon cutting it across, we discover the distinct *laminae* to each angle, rising from a small point in the centre, and separated by a cross, that joins the  
opposite











opposite grooves. Upon putting a thin shaving of it into vinegar, a strong effervescence was immediately raised, which dissolved the gritty or coralline part, and discovered the fine membranes that inclosed it. These two substances seem to compose this bony, ivory, or coral-like stem.

The disposition of the polypes, with regard to one another, is represented by a cross section in the adjoining plate (*see* TAB. XII.) ; where you will observe, that 10 occupy the outward circle, 9 are in the next, and 4 are in the center.

I have learned lately from Mr. Duntze, the gentleman, who presented you with this great curiosity, the following account of it :

That it was taken in the latitude of 79 degrees north ; which is within 11 degrees of the pole, and 80 English miles from the coast of Greenland, by Captain Adriaanz, commander of the *Britannia*, while he was on the whale-fishery last summer. The captain sounding one day in very deep water 236 fathoms, two of them clung to his line. He says the arms or *tentaculi* of the polypes were of a bright yellow colour, and fully extended, when he brought them to the surface of the water ; and made a most agreeable figure, like a fine full-blown flower, which the captain took them for.

I must further observe, that the *Encrinos*, or *Lilium lapideum* of the curious in fossils, so little known before, is thought to be of this class. I am,

S I R,

London, Nov. 8,

1753.

Your most obedient humble servant,

John Ellis.

References

*References to TAB. XII.*

- A*, The cluster'd polype in its natural size, extending itself.
- B*, The same polype, as it was received, after it had been soak'd in water, and the *tentaculi* laid strait.
- a*, The polype in miniature, with its stem of bone or ivory.
- c*, Part of the ivory stem twisted.
- D*, The lower part of the stem, cover'd with a cartilage.
- E*, The cartilage open'd, to shew the tapering of the bony part.
- F*, The cross section, to shew the position of the several bodies of the polype.
- H*, The cross section of the bony stem magnified.
- G*, One of the bodies cut open, to shew its internal muscular form.
- I*, The eggs or spawn in the natural size.
- L*, The same magnified.
- I*, The cuticular covering, which is continued from the bladder at *M* to the cartilage at *E*, or from one end of the stem to the other.
- N*, The indented muscular base, where the bodies of the polype all unite.
- K*, A figure of the *Encrinos*, or *Lilium lapideum*, from *Rosinus*.

XLVII. *Extraëts of two Letters from Father Gaubil, of the Society of Jesus, at Peking in China, translated from the French.*

To Dr. Cromwell Mortimer, M. D. Secretary  
to the Royal Society,

Peking, Nov. 2, 1752.

Read Nov. 15, 1753. **T**HE chronology of China, which I sent to France in 1749, had been long the object of the researches of the late M. Freret, who did me the honour to ask my sentiments upon the essays, which he sent me. This correspondence continued many years; and that learned man wrote several very curious dissertations upon that subject. I have not seen the last, which must have been published in the eighteenth tome of the Memoirs of the Academy of *Inscriptions & Belles Lettres*. I wrote to several persons detach'd pieces of what came into my thoughts on the subject, as they consulted me upon it. To avoid many inconveniences, I thought proper myself to digest my own rough draughts upon that topic; and accordingly sent the result in three parts to France in 1749.

What I have thus done in chronology, I am desirous of doing with regard to astronomy, or, at least, furnishing materials ranged in order upon that science. For it is necessary to draw up anew, or, at least, to correct, what has been printed in the work, which Mr. Costard has criticized in the *Philos. Transact.* N<sup>o</sup> 483, p. 476, 477, & seq. It was not sent from  
hence

hence to France with a view of being printed, but of being examined by men of skill and abilities (a).

I have already sent you the catalogue of some antient observations. What relates to Jupiter is proper to correct what is remarked by Father Souciet. I have sent either to that Father, or to M. de Lisle, a great number of corrections and additions, which are requisite to be made; and I will soon digest into order what shall appear to me necessary. Mr. Costard will there, I believe, see the solution of his doubts. The Jesuits are much obliged to him for the honour, which he has done them, in saying, that the best memoirs relating to China are owing to them. When he shall have examined what there is of reality with respect to antiquity, and the manner of coming at the knowlege of the antient history and astronomy, his doubts will vanish.

The Chinese, without being consummate, or even passable astronomers, might be capable of seeing an eclipse, and of making observations on it, and of looking upon the shadow of the gnomon of a sun-dial. The knowlege, which they had from time immemorial of the rectangle triangle, and of its principal properties,

(a) The Chinese texts were written by men, who knew nothing, or almost nothing, of astronomy and antiquity; and there are many faults in the Chinese contexts, and likewise some in the copies of the translations, and in what was added or corrected for the impression. They are only memoirs, which ought to be examined, and digested into a better order, after having taken the proper measures to restore the Chinese texts, which had been alter'd, and to explain the obscure parts by those, which are intelligible; notwithstanding which there will be many Chinese texts still unintelligible.



properties, might easily teach them a thousand curious things in geometry, without knowing the theory of trigonometry.

The Chinese, from time immemorial, knew the passage of the sun in the ecliptic; they knew the stars; they had globes and hemispheres; and, by means of divers practices and precepts, received from their antients, without any great knowlege of spherical trigonometry, might be able upon the globe itself to resolve many problems. We ought to conclude, that our antients were possessed of several kinds of knowlege, received from the patriarchs, and transmitted to the Chinese. Without these kinds of knowlege, and these traditions, by mere observations alone, the Chinese could not perform what they did at first. They never well understood the stations and retrogressions of the planets. Reflections upon the eclipses of the sun and stars taught them antiently, by practice, something of the parallaxes of the moon.

Every thing was almost forgot, about the time of *Tsin chi hoam*, 240 or 246 years before Christ. But it is evident, that, before that time, the Chinese must have known something of the calculations of the eclipses of the moon and sun, and of some equations for reducing the mean motion to the true, and for calculating the solstices.

*Mengtse*, a classical author, who wrote before the burning of their books, mentions clearly enough part, at least, of what I say. They certainly knew indifferently well the proper motion of the fix'd stars, which was afterwards forgot, for want of examining what was extant written in many books.

I shall

I shall say no more upon this subject; but hope, that Mr. Costard, and many other gentlemen in England, France, and elsewhere, will lay aside their doubts or prejudices about the Chinese astronomy and antiquities. It is, indeed, really difficult, to take the just medium between those, who too highly extol, and those who too much despise, the Chinese literature.

I thought, that Father Hallerstein would not have gone so soon to Macao; and when he was obliged to go thither, the plan of Peking, which I order'd to be copied for you, with some explications of it, was not finished. I have now received it, but I cannot send it by the post with this letter; nor can I send it till next year, when I shall have a favourable opportunity for it. I shall then likewise send some other present for your illustrious Society.

On the 15 of August, an ambassador from the king of Portugal arrived at Macao, with presents for the emperor of China. It were to be wish'd, that this embassy might produce some advantage to us; but this is extremely doubtful. The queen-mother of the king of Portugal order'd the ambassador to desire, that Father Hallerstein, whom she personally knew, might come to him to Macao, with a mandarin sent by the emperor. The emperor consented to this without any difficulty, and dispatched the mandarin and Father Hallerstein to the ambassador. He will be here again in May.

I am of opinion, that the reigning emperor will never permit any missionaries in the provinces; and that they will find it very difficult to conceal themselves. But there is no appearance, that we shall be  
sent

sent away from Peking : on the contrary, those, who shall be sent thither, will be well received, if they have but the qualifications requisite.

The observation of Mercury in the Sun in May 1740 is very curious, and does honour to the calculation of Dr. Halley. I had seen that calculation; and doubting that of Manfredi in his Ephemerides from the tables of M. Cassini, I prepared to observe that passage of Mercury here at Peking; but the weather was not favourable. And I see, by the observation made at Cambridge near Boston in New England, that I might have had one at Peking. I hope, that we shall be able to observe the passage of Mercury in May next year. It is of great importance, and we shall endeavour to do it in the best manner; and if we succeed, we shall communicate it to you.

## II.

To Monf. de L'Isle of the Royal Academy of Sciences at Paris.

S I R,

Peking, Nov. 18, 1751.

**Y**OU know, that I have, for many years, corresponded with the late famous Monsieur Freret, [Secretary of the Academy of Inscriptions and Belles Lettres] who honoured me with his friendship. Monsieur de Bougainville, his successor, wrote to me last year, and acquainting me with the melancholy news of his death, informed me, that he was just ready to commit to the press a great work of that eminently learned man upon the Chinese chronology. I had seen some essays of it in the Memoirs of the Academy of Inscriptions and Belles Lettres, of which

he made me a present. I had furnished him with a quantity of memoirs, as I had likewise done to others, both seculars, and those of our own society. I digested into order all that I had collected; and, in 1749, sent a complete treatise on the Chinese chronology, by two different ways, into France. I directed it to M. Freret, and to the Fathers of our society at Paris. It was in three parts. I desired them to communicate it to you, and to Mons. de Mairan. I have had no account of the arrival of that treatise, in which I had labour'd for above two-and-twenty years past. It seem'd to me necessary, on account of the great number of pieces, either printed or manuscript, which were sent hither on that subject. If I find, that my treatise is lost, I can easily digest it into order again, from the rough draught, which I have by me.

Besides many astronomical observations, which I have punctually sent you, I have transmitted to you the treatise of Father Duchamp upon the Indian astronomy, a collection of antient approximations and occultations of the stars and planets, both by each other, and by the moon, and with the moon; which I had collected and made for determining the longitude and latitude of Peking, &c. This year I have sent to Paris, by two different ways, a memoir, which had been desired of me, concerning the isles of *Lequoyo*, or *Licoukicou*, which Kempfer calls *Roukou*. I have directed, that this memoir should be communicated to you. It is a pretty long one. I had an opportunity of being well informed about these isles; but there are many things yet wanting to be known. To this memoir I have added some remarks concerning the longitude of *Namgazaki*, and other places on the south

south coast of Japan, and the south coast of Corée, with its distance from Japan, and the island of *Tou-yma*, which, in the map of Father du Halde, is called *Touyla Tao*, or the island of *Touyla*. It should be called *Touy Ma*. It is the isle *Tsutsuma*. It depends upon Japan. I have spoken here with several Coreans, who have been in that island.

I have already sent to you observations made here to the close of the year 1750, and during this year. I now send you others of 1750; and others I inclosed to you at large in 1749 and 1750. I wait for some answer from you; and especially your opinion concerning the manner, in which I ought to dispose my memoirs concerning the Chinese astronomy. I am resolved to put my last hand to that work. But memoirs of that kind ought to be examined by persons intelligent and zealous like yourself.

At Petersburg you must undoubtedly have seen what I wrote to Mr. Bayer about what the Chinese have said concerning the Huns and Turcs. Dr. Mortimer has written to me, that he had received from a nephew of Mons. Fourmont a small piece upon the origin of the Turcs and Huns, as drawn from the Chinese books. I shall speak again of that subject in the memoirs, which I have of the history of the Great Dynasty of *Tang*. There are a great number of very interesting things upon what the Chinese have delivered at that time concerning the empire of the Persians, and its destruction by the Mahometans; concerning the Mahometans, and the assistance, which they gave to Chinese emperors against their rebels; concerning the Christian religion, or the *Tatfin*, but in very obscure terms; concerning the sects and countries of the

Indians, Japan, Corée, Tartary, and the countries between China and the Caspian Sea, Tybet, and its princes. All these particulars may be of considerable service to unravel the eastern history from the year 500 of Christ to the year 1000 after him, and even much higher. If I have not time to reduce my memoirs into proper order, I will address them to some learned man, like yourself, who can digest them, or see them digested.

It is several years since I received any thing from Father Boudier. He has, undoubtedly, sent every thing to Paris. However, I have a good part of what he did, till 1738, and 1739; and I find, that he is much mistaken with respect to the diameter of the sun, and the obliquity of the ecliptic. I do not know whether the right ascensions of the stars are very exact. He had not then any knowledge of the aberration of the stars. His voyage from Chandernager to Agra, Dheli, Jaepour, is curious. I have the chart with the observations of the latitude and longitude of Fatepour, Agra, Dheli, Jaepour, and Chandernager. I could have wish'd to have had the exact distance from Agra and Dheli to the Ganges.

I have discoursed here with some religious Indians, who have been at Sirnegar, and as far as the sources of the Ganges; but their account is very obscure.

There are here a great number of Lama's and Tartars, who have gone from Lassa, the capital of Thibet, to the lakes and mountains, where the sources of the Ganges are, and at Latac, &c. in the country to the north of Thibet and Latac; but what they say is extremely confused; and this part of geography is still very little known to us here. It would

be

be of great use to us, if you are informed of it, and would do me the pleasure of giving me an account of it.

Mr. Hodgson has sent hither, to the two German Fathers, his theory and tables of the satellites of Jupiter. There are a great number of observations made at Peking; many of which are not certain, nor proper for the forming of tables. The catalogue of the longitudes and latitudes of places is very erroneous. He will undoubtedly correct them. However, the work itself is a very good and useful one. Yourself and the other astronomers are able to judge of it much better than I can here.

The *Memoires de Trevoux* speak in a manner, which, they say, they had learned from you, concerning the rotation of the axis of the earth, and the consequences, which Dr. Bradley draws from it. It is a discovery worthy a man of his profound sagacity; but will render it necessary to make a vast number of new calculations.

XLVIII. *A Letter of Mr. William Sher-  
vinton to Benjamin Franklin, Esq; of  
Philadelphia, concerning the Transit of  
Mercury over the Sun, on the 6 of May  
1753, as observed in the Island of Anti-  
gua: Communicated by Mr. Peter Collin-  
son, F. R. S.*

S I R,

Antigua, June 20, 1753.

Read Nov. 15,  
1753.

**M**R. Benjamin Mecom having re-  
ceived half a dozen circulatory  
letters from you relating to Mercury's transit over  
the sun the 6 of last May, he put them into my  
hands. One would have sufficed for our island, as  
we are not overburthen'd with men, who have a taste  
that way. Hereunder I send you the result of my  
observation \* thereof.

Sunday, May 6, at 6<sup>h</sup> 7' 51", I observed the  
western limb of Mercury to touch the western limb  
of the sun; and, at 6<sup>h</sup> 10' 37", he touch'd the same  
with his eastern limb, and totally disappear'd. Lat.  
of the place 17° 0' N. Lon. by estimation 61° 45'  
W. from London.

This was taken by a Graham's watch, and cor-  
rected by two altitudes taken by a most exquisite  
quadrant; viz.

At

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\* Dr. Charles Rose, who was in Antigua at this time, says, that  
these observations were taken by Capt. Richard Tyrrel, of the said  
island,



At 6<sup>h</sup> 58' 7", I observed the distance of the sun's upper limb from the zenith = 72° 21' 30". And, at 9<sup>h</sup> 31' 5", I observed the same = 36° 17' 0".

By the common process (which you may have, if necessary) I found the watch was 0° 4' 4" 28''' too fast \*; therefore,

					h	'	"	'''
From	—	—	—		6	10	37	
Take	—	—	—		0	4	4	28
					<hr/>			
True appart. time of Merc's. exit here,					6	6	32	32
					<hr/>			

Pray impart your observation to

Your Well-wisher,

William Shervington.

island, and who is possessed of a valuable collection of astronomical instruments, made by Mr. Bird in the Strand, London, and that Mr. Shervington only was present.

\* Mr. Shervington has taken the mean of these two altitudes from the error of his watch; and there can be no doubt that his observation is a good one, which, compared with that made in Surry-street by Mr. Short, *p.* 199, *l. i. &c.* fixes the longitude of the place of his observation in Antigua 4<sup>h</sup> 5' 30'', or 61° 22' 30'', west of St. Paul's, London.

XLIX. *An Account of the Barometer, and the State of the Weather, at Dublin, from the 7 of March 1752, to the 28 of February 1753, by James Simon, Esquire, F. R. S. and Secretary of the Incorporated Society at Dublin : Communicated by Mr. Henry Baker, F. R. S.*

Dear Sir,

Dublin, Sept. 4, 1753.

Read Nov. 15, 1753. **I** NOW send you my journal of the weather for the last year, from the 7 of March, 1752, to the 28 of February, 1753. You will, perhaps, find it too minute; but whatever fault you find in it shall be rectified for the future, if you are so kind as to let me know, how I should proceed to render it useful. Deal with me as a friend; inform me of my mistakes; and if you do not think it deserves to be laid before the Royal Society, I beg you will not let it be seen; if otherwise, you have my leave to lay it before them. My chief ambition would be to deserve their approbation and esteem: of this you may assure them, and of my sincere respect and regard.

I have observed, that the friction is so great in the common barometer, by the smallness of the tube, that I wrote to London for a larger tube. It is something more than one quarter of an inch clear in the bore; and it took me two pounds and a quarter of clean purified mercury to fill it. The cistern, which receives the mercury, is pretty large, with a wide mouth; and is cover'd with a silk gauze, besides the usual







usual wooden cover, to prevent any dust from falling into the quicksilver. In such a tube the friction is much less; and I can observe the most sudden change in the weather, even to an 80 part of an inch; tho' in my journal I only set down a 40 part. For example, March 12, 1752, at night, 29  $14\frac{1}{2}$ , is 29 inches  $14\frac{1}{2}$  twentieth parts and a half of an inch, or  $\frac{29}{40}$  parts. I am,

Dear Sir,

Your most humble servant,

James Simon.

*L. A second Account of the new Method of opening the Cornea, for taking away the Cataract; by Samuel Sharp, Surgeon to Guy's Hospital, and F. R. S.*

Read Nov. 22, 1753. **I** HAD the honour, last April, to lay before the Society a new method of opening the cornea, in order to take away the cataract; being intended as an improvement on Monsieur Daviel's manner of doing that operation: and having now practised it on several subjects, I here take the liberty to give a short account of the success, with a few observations on the principal phænomena attending this operation; to which I shall add a description of a farther improvement of the operation itself. For a fuller view of the history of these cases, I have here set down the ages of the patients, the dates of the days on which they underwent the operation, and the particular circumstance of its being done on one or both eyes.

Names.	Age.	Date.	Eyes.
<i>A B</i>	55	April 7	1
<i>A C</i>	54	April 16	2
<i>A D</i>	70	May 12	2
<i>A E</i>	65	June 6	2
<i>A F</i>	65	July 16	2
<i>A G</i>	63	August 8	2
<i>A H</i>	48	Sept. 19	2
<i>A I</i>	64	Sept. 19	1
<i>A K</i>	67	Sept. 19	2
<i>A L</i>	58	Oct. 4	2
<i>A M</i>	47	Oct. 22	1

From

From this catalogue it appears, that the operation has been performed on nineteen eyes; and, from the most exact information, which I have been able to procure, the state of the success stands thus:

*AC, AD, AF, AG, AL*, all which had the operation performed on both eyes, have every one of them recovered the sight of both eyes, to as great a perfection as can be supposed, without the help of the crystalline humour; that is, they can read and write, with proper spectacles.

The first of them, *AC*, has found so much benefit, as to be able to carry on the exercise of his profession, that of a surgeon.

*AH* sees with both eyes, but not so well as the other five. I have just now an account from the surgeon, who has attended her (in a distant country), that her eyes look well, and her sight improves\*.

*AI*, another patient, at a distance from London, had the operation done on one eye only; which he recover'd, as my correspondent informs me, so as to see tolerably well.

*AM*, on one eye only, with which he already sees very well.

*AE* had it performed on both; one of which was lost, and the other recovered; but continues inflamed, and cannot bear much light.

*AB* had it done on one eye only, which was lost.

S f 2

Both

\* Some weeks after this paper was read, Mr. Sharp received an account, that the pupils of both eyes had lately contracted so much, as hardly to leave room for the admission of light; and it was apprehended the patient would soon become blind.



Both the eyes, in which the operation fail'd, were destroy'd by the subsequent inflammation: but, to do justice to this new operation, I might here take notice, with regard to the case of *A B*, that the ill success was partly owing to the imperfection of my instrument; a disadvantage, that must frequently attend on the execution of new attempts. It was the first operation I performed, and I had provided a knife with so thin a blade, that, after I had passed through the cornea into the anterior chamber of the eye, the point was so blunted, that, upon endeavouring to carry it through the cornea out on the other side, the blade bent, and I was apprehensive it might break: however, withdrawing it a little, I made two or three efforts, and succeeded in the incision, and the removal of the cataract.

During this operation, the aqueous humour being discharged, and the patient struggling, I wounded the iris; which bled profusely, and continued for several days to discharge a great quantity of blood, and bloody ichor: and it is to this accident that I am inclined to impute the miscarriage of the operation; though Mr. Daviel affirms, that the wounds of the iris have been very seldom followed with bad effects in his practice.

I have reserved the mention of *A K*'s history to the last, because of its singularity. She was altogether as blind as those, whose cataracts are ripe; but hers had the appearance of a beginning cataract, being of a light blue, and but little opaque. Upon making the compression, the crystalline did not advance through the pupil, as in the other instances: and I  
found,

found, that, if I exerted more force, I should soon evacuate all the vitreous humour. It was evident, by the great distance of the cataract behind the iris, that this disappointment did not arise from an adhesion to the iris: however, I had immediately recourse to the experiment of cutting through the capsula with the point of my knife; hoping, by that means, to have set free the crystalline, but it gave me no assistance. I then passed the *curette* (a little scoop) through the pupil, and turn'd it several times round, in expectation of breaking the capsula; but found not the least resistance to my instruments; so that both operations proved ineffectual; the circumstances being exactly the same in each eye.

I have, in couching, met with cataracts of this nature; but I had no apprehension that I could not have discharged, by the wound of the cornea, the matter of a cataract, in however fluid a state it might prove.

Of all the 19 there was not one, that escaped an inflammation; whereas, after couching, there are great numbers, who have neither inflammation nor pain. But it is to be remarked, that, notwithstanding the violent inflammation, which sometimes ensues after the incision of the cornea, even to an enlargement of the eyelids, and vesication of the *tunica conjunctiva*, the patient complains rather of a tenderness of the eye, upon touching it, than of pain; being generally exempt from those dreadful dartings in the head, which, for the most part, accompany an inflammation after couching. And I believe I may assert, that none suffer'd very much in that particular, except

cept *A E*, who was extremely bad, and lost the eye on that side, where the pain was.

I presume it will not be difficult to conceive, how it should happen, that these inflammations should excite such different symptoms, when we reflect, that, in the incision of the cornea, the cornea only suffers; and in couching, the *conjunctiva*, the *sclerotica*, the *choroides*, and the *tunica retina*, are punctured; most of which organs are either tendinous or nervous; and every surgeon knows the painfulness and obstinacy of inflammations, when they follow upon wounds and punctures of tendinous or nervous parts.

I have not mentioned, in this comparison, the violence done to the vitreous humour; because I believe it does not occasion the subsequent pain; and because it seems to be often as much or more injured in the new operation, without any notable inconvenience.

It has not occur'd in any of these cases, that the inflammation has been so slight, as to disappear intirely in a fortnight, or three weeks; most of them requiring six weeks, and some longer, for the total removal of them. The first ten days, or more, the light is generally very offensive; and I have observ'd, in three or four instances, that, upon forcibly opening the eyelids during that time, the patient was only sensible of a glare of light, though the eye then appeared clear, and he afterwards recover'd his sight. Which I mention, to obviate the melancholy prognostic one would be disposed to make upon a first examination. However, this is not to be understood as a constant fact; some patients distinguishing objects immediately from the time of the operation.

It

It sometimes happens, after this operation, that the pupil loses its circular figure: which, I imagine, is owing to the great tenderness of the iris, that, upon the least violence, is subject to be ruptured; and, I suppose, in this operation, a slight pressure from the back or the flat of the blade may have produced the accident in the instances alluded to. Possibly the sudden dilatation of the pupil, from the rapid passage of the cataract through it, may sometimes occasion it; but the following history would induce one rather to ascribe it to the cause, which I first mention'd.

Before I had thought of the knife for opening the cornea, I used the scissars, as Monsr. Daviel directs; and, in a certain patient, after I had made the wound of the cornea, and was going to compress the eye, for the expulsion of the cataract, I discover'd, that, from the disturbance I had given to the humours by the foregoing process, it was sunk almost as much as if it had depressed by a couching needle. I therefore left it in that situation, and the man afterwards saw very well; tho' the cataract remained visible something below the pupil.

Now, in this instance, the cataract had not passed thro' the pupil; and yet it was lacerated, so as to lose its circular form: but, whatever may be the cause, I do not find, that the accident itself proves prejudicial to the sight.

I shall finish these observations with a remark on one disadvantage, to which this operation and couching are incident; and that is a possibility of an incomplete *gutta serena* being complicated with the cataract. It has, indeed, been a rule with surgeons

tho' the cataract appear fair, never to perform the operation, but when the patient is sensible of a glimmering of light; because a total privation of sight argues some other disorder of the eye. But the criterion is not infallible; for it may happen, that a *gutta serena* shall prevail to a degree, that does not take away the sense of light, and yet prevents the distinction of objects: So that when a cataract is thus circumstanced, the operation will be fruitless.

It remains now to speak of the operation itself. In my former paper, after having described the manner of making the incision, I directed the operator to compress the inferior part of the globe of the eye with his thumb gently, till the cataract should be expell'd through the incision of the cornea, upon the patient's cheek; and in this method I have perform'd it upon several subjects. But remarking, that tho', upon the evacuation of the aqueous humour, the crystalline readily advanced through the pupil into the anterior chamber, yet that it required some force to expel it from its membrane through the wound of the cornea, and in that action it sometimes suddenly drew after it a portion of the vitreous humour, I changed my method, and no longer press'd the eye, when once the crystalline was in the anterior chamber, but immediately stuck the point of my knife into the body of it, and extracted it contained in its capsula, without spilling any of the vitreous humour.

This new process, I suppose, will be found of considerable advantage, as it will, in a great measure, remove the danger of evacuating the whole, or too much, of the vitreous humour: though it may be observed, to the praise of this operation, that, contrary

trary to expectation, a large quantity of this humour (perhaps a third part, or more) has been sometimes discharged, without any bad consequence.

I have supposed, that the great and sole benefit arising from this improvement, is the easy separation of the crystalline from the bed of the vitreous humour, so that none of the vitreous humour shall be evacuated. But perhaps it will also be approved of, as it will render unnecessary the measure prescribed by Monsr. Daviel, of wounding the membrane of the crystalline, before we proceed to the extraction of the crystalline itself: To which purpose he advises the flap of the cornea to be suspended with a small spatula; then, with a pointed cutting needle, to wound the surface of the crystalline; after which, to introduce the same spatula through the pupil, in order to detach the cataract from the iris, and then proceed to the expulsion.

I have here recited these processes of M. Daviel's operation, which are calculated merely to procure an easy operation of the crystalline from the vitreous humour: But they are difficult to the operator, fatiguing to the patient, and, I should hope, altogether needless, if the knife be used in the manner which I have recommended: For, whether the capsula of the crystalline be nothing more than the duplicature of the membrane of the vitreous humour, or whether it be a proper coat, which is also cover'd by the membrane of the vitreous humour; in either case, since by compression the chrySTALLINE advances with so much facility through the pupil, it will be easily seized by the knife, and removed from the vitreous humour, with its enveloping membrane:

T t

Whereas,

Whereas, in making an incision on the surface of the crystalline, and wounding its capsula, the crystalline will frequently slip out of the capsula, which will be left behind: And, in fact, this has happen'd to M. Daviel, who advises pincers, and other instruments, in order to extract the remaining membrane. However, I shall here observe, in regard to the capsula of the crystalline, that, should the humour slip out of it, before it be seized by the knife, it possibly will waste; for, in milky cataracts, when the fluid is discharged, the membrane, in length of time, wastes: Whole cataracts, with the enveloping membrane likewise, sometimes waste: And in one of my patients (*A. C.*) the crystalline, from the mere pressure in the operation, burst out of its capsula, which I left in the eye; but in some weeks it intirely wasted. However, if the removing of the capsula should, by future experience, be found necessary, it may be conveniently done by the *curette*; one of the instruments M. Daviel recommends upon that occasion. This instrument may be also used for the extraction of a cataract, which has been broken to pieces by the couching needle in a former operation, and for the removal of the capsula of bag-cataract, when the fluid only has been discharged, and the bag remains behind; but it will be most eminently useful in detaching the crystalline from the back part of the iris, when any portion of it happened to adhere: Which circumstance would render the operation fruitless, without such a precaution.

It has not happened, in any of the cases I have treated, that, either during the operation, or after the operation, the iris has been push'd forwards, or  
 insinuated

insinuated itself through the wound of the cornea, forming a *staphyloma*; but M. Daviel speaks of it as an occurrence he has met with, and says it may be easily replaced by the small spatula.

I presume a greater number of operations will prove this account very deficient: But I have here communicated all that I have done, and all that I know on the subject; not having suppress'd one experiment, nor, to the best of my remembrance, one circumstance, either to the honour or disgrace of the operation.

It is to be hoped, that, when it shall be more generally practis'd, ingenious men will render it still more perfect: And I should not be surpris'd, if the use of a *speculum oculi* should hereafter be esteemed an improvement: But then it must be contriv'd so, as that it shall not compress the globe of the eye; or, if it does, the operator must be careful to remove it in the instant the incision is making, lest, by continuing the pressure after the wound is made, all the humours should suddenly gush out.

It was impossible for me to make any remarks on this operation, without criticising on M. Daviel, as M. Daviel has not only the honour of having first practis'd it, but is the only writer who has treated the subject, at least that I am acquainted with. I therefore flatter myself, that this attempt to improve upon what he has laid down, will not be construed as a reflection on him, or his practice: For, however his invention may be perfected by others, in my opinion, it is still to him principally, that the world will be indebted for the benefit of the discovery.



LI. *An attempt to explain an antient Roman inscription, cut upon a stone lately found at Bath.* By John Ward, LL.D. *Rhet. Prof. Gresh. and V. P. R. S.*

Read Nov. 22, 1753. A COPY of this inscription was first communicated to this Society by Mr. *Henry Baker*, who received it in a letter from *John Browning Esq*; dated the fourth of July last. And the Society being then pleased to refer it to my further consideration, several other copies of it were in a few days sent me by my freinds from *Bath*, and other places. But upon comparing them one with another, I found they all differed in some respects; excepting two only, one of which had been copied from the other. And therefore, as I could not by this means settle the reading to my satisfaction, Dr. *Richard Davies* the physician being then in *London*, I acquainted him with the affair; who was so obliging, as to promise me, that upon his return to *Bath* he would procure for me an exact copy from his own inspection. And accordingly he applied himself for that purpose to the ingenious statuary at *Bath*, Mr. *Prince Hoare*; who apprehending that a cast of it in plaister of *Paris* might express both the form of the letters, and the defects of them, in their present state, with greater exactness than a written copy, was so kind as to take one off from the stone; which I received afterwards from Dr. *Davies*.

The stone was discovered upon the twenty-second of June last, about five feet under ground, in digging the cellar of a house, which was rebuilding at the lower

lower end of *Stall Street*. Among the rubbish of the old house, when it was pulled down, was a large quantity of walling stone, which had on it the marks of fire; so that probably some building had formerly stood there, which was burnt. And in sinking the ground about four or five feet lower than the stone, they found, as I am informed, two coins of the emperor *Carausius*, in base metal, and very much defaced; tho upon one of them the following inscription was legible: IMP CARAVSIVS PF. In July 1727 the beautiful gilt head, which is now preserved in the town house, was dug up at the other end of this street, not far from the *King's bath*, about sixteen feet below the surface of the earth, as they were making a common shore through the town (1).

The stone, upon which this inscription is cut, has been generally taken for a pedestal, either of a statue, or some other solid body, which it once supported. Tho from the appearance of the horizontal plane at the top Mr. *Hoare* is of the opinion, that nothing was formerly placed upon it; and supposes, that the sinking in the middle, with the two lines erased, one on each side, might be made meerly for ornament. Besides, the face and two sides only are finished; the back being flat, as if it was designed to stand against a wall. The height of it, which is very near three feet; as likewise the form both of the stone itself, and the plane above mentioned; will appear by the draughts of them taken by Mr. *Hoare*, which are annexed to this discourse (2). From

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(1) A fine print of this head has been published by the A. S. L. in their *Vetust. Monum. Vol. I. Num. 34.*

(2) See TAB. III. Fig. 3.

a careful examination of the whole inscription, as it appeared to me in the cast, I have copied it in the draught of the stone; and indeavoured to express the several letters in their proper form and proportional size, together with the ligatures, divisions of the words, and their situation in each line, in the most exact manner I was capable of doing it. And upon considering the whole in this view, I take leave to offer the following reading in words at length, as what appears to me the most probable.

*Locum religiosum, per insolentiam erutum,  
virtuti et numini Augusti repurgatum  
reddidit Caius Severius Emeritus, centurio,  
sua pecunia.*

1. That the words *LOCVM RELIGIOSVM* must here signify a *burying place*, cannot, I think, be well doubted of; since that is the usual acceptation of them in the Roman laws. Thus *Marcian* says: *Religiosum locum unusquisque sua voluntate facit, dum mortuum infert in locum suum* (1). The same thing is said by *Justinian* (2). And in like manner *Ulpian* says: *Locum, in quo servus sepultus est, religiosum esse Aristo ait* (3). Moreover, as a severe punishment was inflicted on those, who violated the sepulchers of the dead (4); so for their further security they

(1) *L. 6. § 2. D. de divis. rer.*

(2) *Instit. § 9. eod.*

(3) *L. 2. princ. D. de religios. et sumpt. fun.*

(4) *L. 3. § 7. D. de sepul. violat.*

they were consecrated to the *Dii Manes*. And as temples and altars, dedicated by public authority to the *Dii Superi*, were stiled *loca sacra* (1); hence, as *Aggenus Urbicus* observes: *Sacrum proprie Dei est, religiosum hominum* (2).

It is well known, that by the laws of the *Twelve tables* burials were prohibited within the city of *Rome*. The words of the law are these: *Hominem mortuum in urbe ne sepelito, neve urito* (3). The like prohibition was afterwards extended to the provinces, by the emperors *Hadrian* (4), *Antoninus Pius* (5), and others (6). There were indeed some exceptions to this law, in favour of particular persons, which do not come under our present consideration. The *Romans* therefore commonly placed their sepulchres, either near the common roads, or within their own possessions, or in some place allotted for that purpose by authority. Instances of the first sort are to be met with in *Gruter*, and other writers upon this subject. And the reason of it is assigned by *Varro*, who says: *Monimenta in sepulchris ideo secundum viam sunt, quo praetereunteis admoneant, et se fuisse, et illos esse mortales* (7). As to private or family sepulchres, a very full and ample account of them has been given by the late *Roger Gale* esquire, a worthy member

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(1) *Festus in voce Religiosus. Instit. § 8. ubi supra.*

(2) *De controvers. agrar. pag. 61. edit. Gæf.*

(3) *Tab. x. L. 2. princ.*

(4) *L. 3. § 5. D. de sepul. violat.*

(5) *Jul. Capitol. in vit.*

(6) *L. 12. C. de religios. et sumpt. fun.*

(7) *De L L. Lib. v.*

ber and treasurer of this Society, in a discourse written by him upon that subject, and formerly printed in the *Philosophical Transactions* (1), to which I need only refer. The place at *Rome* most remarkable for the sepulchral monuments of illustrious persons, as appears from *Strabo* (2), was the *Campus Martius*, where they were buried by order of the senate. But the common burying place allotted for slaves, and other mean persons, is by *Varro* called *Puticulae*; which lay, as he says, beyond the mount *Esquiliae* (3). And to this *Horace* alludes in the following verses :

*Hoc miserae plebi stabat commune sepulchrum,  
Pantolabo scurrae, Nomentanoque nepoti* (4).

And that the like custom obtained in other parts of the Roman empire, appears from a passage in *Aggenus Urbicus*, in which he says: *Loca autem, quae sint publica, videamus*. And then, after some others there mentioned, he adds: *Sunt in suburbanis loca publica, inopum destinata funeribus, quae loca culinas appellant* (5). Where under the word *inopum* must be included all such persons, who had no private or family burying places of their own; unless some particular place was assigned them by order of the

(1) *Num.* 441. pag. 211.

(2) Διότι οἱ ἱεροπρεπῆς αὐτὸν νομίσαντες τὸν τόπον τῆτον, καὶ τὰ τῶν ἐπιφανέστερων μνήματα ἐν ταῦθα κατεσκευάσαν ἀνδρῶν καὶ γυναικῶν. *L. v.* pag. 236. edit. Casaub.

(3) *De L. L. Lib.* iv.

(4) *Lib.* i. *Sat.* 8. v. 10.

(5) *Ubi supra*, pag. 60.

the magistrates, which was not uncommon, as we find by several antient inscriptions (1).

But there was no town, to which this could be more suitable than *Bath*, on account of the great number of strangers, who resorted thither for the benefit of the salutiferous springs. For as some of those, who came from distant parts, may be supposed from time to time to have died there; a public coemetry for the burial of them was highly requisite. All the Roman inscriptions hitherto found at or near *Bath* have been of the sepulchral kind; except that now under consideration, which however has some affinity with them. The rest are five in number; four of which were first published by *Camden* (2), and since by Dr. *Guidott* (3) and others; and the fifth, relating to *Julius Vitalis*, by Dr. *Musgrave* (4). But of the four in *Camden*, two at least appear to have belonged to persons, who were not settled there, but came from distant places, probably upon the account of their health. One of these is called *decurio coloniae Glevensis*, that is, a *senator* or *aldermen* of *Gloucester*, which was then a Roman colony named *Glevum*. The other is said to have been a soldier *legionis secundae adjutricis*, as those words have been generally read. But, as *Horsley* has remarked, it does not appear, that this legion ever was in *Britain* (5). As most of those inscriptions were found at *Walcote*, a village

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(1) See *Sertorii Monument. Patavin.*

(2) *Britann. pag. 171, 172. edit. 1607.*

(3) *Discourse of Bath, Chap. x.*

(4) *Julii Vitalis Epitaphium.*

(5) *Britann. Roman. pag. 326.*

village upon the *Fosse road*, about a mile north east from *Bath*; the coemetry is supposed to have lain at or near that place (1), where, as *Dr. Stukeley* observes, there was also a Roman camp (2).

2. The next words in the inscription, *PER INSO-  
LENTIAM ERVTVM*, are attended with no small difficulty. Some persons, as I was informed, had taken the first letter of the last word for a *D*, which led them to read it *dirutum*; tho there is no appearance of an *I* after the *D*, nor indeed room for it upon the stone, to support that reading. But on viewing the cast, the perpendicular stroke of that letter seemed to me to be carried somewhat higher than the other letters of that line, which stood before it. And this occasioned me to suspect, that it might have been designed for an abbreviation of the two letters *D I*, as in another of our British inscriptions, where those two letters are so combined in the word *BLECTI* (3). But having consulted *Mr. Hoare* concerning it, he informs me, that upon a reinspection he thinks it to be an *E*; for which reason I have written it *erutum*. However, this will make no difference in the sense; for so *Tacitus* uses *eruere* for *diruere*, when he says: *Cerialis postero die coloniam Trevirorum ingressus est, avido milite eruendae civitatis* (4).

When this inscription was last before the Society, a learned gentleman then present was pleased to query, whether the words *per insolentiam* might not here signify

(1) See *Musgrave, Jul. Vital. Epitaph. p. 172.*

(2) *Itiner. curios. pag. 140.*

(3) *Britann. Roman. York, XIII.*

(4) *Hist. Lib. IV. cap. 72.*

ignify *through disuse*. In relation to which I would beg leave to observe, that there are indeed some passages in the Roman writers, which may seem to favour such a sense. So *Turpilius* the poet: *Quid mihi vellem, ex insolentia nesciebam* (1). And *Cicero*: *Non superbia, sed istius disputationis insolentia, atque earum rerum inscitia feci* (2). And again: *Moveor loci insolentia, quod tantam causam dico intra domesticos parietes* (3). But in each of these places, and some others, which might be mentioned, the word *insolentia* refers to what never had been used; and not what ceased to be so, as the sense would here require. Besides, it does not seem to suit with the participle *erutum*, with which it is joined. For tho a building may be said to fall down, and come to ruin, through neglect or disuse; yet it is not, I think, usual to say, that it was pulled down or demolished, meerly by length of time, or from want of care to support it. The Latin word commonly used in that case is not *erutum* or *dirutum*, but *collapsum*. And so we find it expressed in another of our British inscriptions: *Templum olim vetustate conlapsum Gaius Julius Pitannus, provinciae praeses, restituit* (4).

This coemetery therefore, as I imagine, had been plundered, and reduced to a ruinous state, by some illegal acts of fraud or violence. Nor does this seem to have been a very uncommon case, notwithstanding

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(1) *Apud Nonium in voce Insolens.*

(2) *De orat. Lib. I. cap. 22.*

(3) *Pro Dejotar. cap. 2.*

(4) *Britann. Roman. Cumberl. xxxiv.* See likewise *Northumb.*



ing the many Roman laws yet extant, which were made for their security (1). For as *Aggenus Urbicus* remarks, speaking of such licentious practices: *Ex his locis, cum sint suburbana, sine ulla religionis reverentia solent privati aliquid usurpare, atque hortis suis applicare* (2). What is here referred to by this writer, is sufficiently explained by *Julus Paulus* in the following passage: *Qui monumento lapidem columnamve sustulerit, sepulchrum violasse videtur* (3). Another inducement for plundering sepulchers might be the expectation of finding money, or other things of value, deposited in them. Among the several prohibitions against expensive funerals, enjoined by the laws of the *Twelve tables*, it is said: *Neve aurum addito* (4). This indeed was very suitable to the frugality of those antient times. But that it was not constantly attended to afterwards, is very evident from the many laws, which were made by several emperors in succeeding ages to prevent the burying of money in sepulchers; which laws would have been unnecessary, if that practice had not continued. And it has sometimes happened, that coins, rings, bracelets, and other small ornaments, have been found in Roman urns, both in this and other countries. However, as this practice was strictly speaking illegal, those, who had a property in the sepulchers, were permitted to take away the money found in them; tho  
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(1) *D. Lib. XLVII. tit. 12. de sepul. violat. And C. Lib. IX. tit. 19. eod.*

(2) *Ubi supra,*

(3) *Sentent. recept. Lib. I. tit. 21. § 8.*

(4) *Tab. X. cap. 11.*

it was judged highly criminal and impious for other persons, upon any account, to disturb the ashes of the dead (1). But I shall now proceed with the inscription.

3. The next words, *VIRTUTI ET NUMINI AVGVSTI REPVRGATVM REDDIDIT*, acquaint us with the method, which was taken to redress this ruinous state of the burying ground. And this, I presume, by the word *repurgatum* here used, was done by removing the rubbish; repairing the sepulchers, which had either been plundered, or in any measure demolished; and very probably renewing the fence, which in such places was usually a wall of stones, laid close upon one another without mortar, called by them *maceria*.

But this, it seems, was not thought sufficient for its future security, without putting it under the immediate protection of the emperor, as its tutelar deity; for nothing less than this could, I think, be meant by the words, *virtuti et numini Augusti reddidit*. The word *numen* in its primary sense signifies the *divine power*, as when *Cicero* says: *Omnes naturae numini divino parent* (2). And elsewhere he thus addresses himself to the body of the Roman citizens: *Vos, quorum potestas proxime ad deorum numen accedit* (3). But soon after, upon the change of the government, when the emperors were revered as deities, their power is likewise expressed by the term *numen*, as rivaling that  
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(1) See *Kirchmann. De funer. Rom. Lib. III. cap. 24.*

(2) *De natur. deor. Lib. I. cap. 9.*

(3) *Pro Rabir. cap. 2.*

of the gods. And as words, which denote the properties of things, are often made use of to express the things themselves; so *numen* is sometimes used in a personal sense for a deity. Thus *Horace* complimenting *Augustus* upon the divine honours, which were paid him by the Roman people, says :

*Laribus tuum  
Miscet numen (1).*

Nor was this extravagant flattery peculiar to the poets; for, as *Tacitus* informs us: *Rubrio crimini dabatur violatum perjurio numen Augusti (2)*. And in another place he says: *Fortunam, et deos, et numen Othonis adesse testabantur (3)*. We find the like use of the word also in *Valerius Maximus*, who speaking of some persons, who falsely pretended to have sprung from noble families, thus expresses himself: *Nec divi quidem Augusti, etiamnum terras regentis, excellentissimum numen intentatum ab hoc injuriæ genere extitit (4)*. But this occurs most frequently in their monumental inscriptions. So among many other instances in *Gruter*, there is one upon an altar at *Narbo*, which has this title :

NUMINI. AVGVSTI. VOTVM  
SVSCEPTVM. A. PLEBE. NARBO  
NENSIVM. IN PERPETVOM (5).  
This

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(1) *Carmin. Lib. iv. 5. 34.*

(2) *Annal. Lib. i. cap. 73.* Where see the Commentators.

(3) *Hist. Lib. ii. cap. 33.*

(4) *Lib. ix. cap. 15. § 3.*

(5) *Pag. ccxxix.*

This altar is there said to have been erected in the consulship of *T. Statilius Taurus* and *L. Cassius Longinus*; that is, in the year of Rome 763, as *Pigbius* places it (1); or 764, according to cardinal *Noris* (2): which shews it to have been done, while *Augustus* was yet living. And that the same practice continued in the time of the following emperors, is evident from several of our British inscriptions, where the like compliment is paid to them. So we meet with *Numen domini nostri Augusti*, *Numen imperatoris*, and *Numen principis optimi* (3), with other variations in the manner of expression. From hence therefore, I think, we may conclude, that by the words *virtuti et numini Augusti* in this inscription, not only the prowess or fortitude of the emperor then reigning, but likewise the divine power here ascribed to him, is called in aid for the defence and preservation of this coemetery. As to the form of the expression here used, things were said *diis reddi*, which were offered or consecrated to them. Thus *Suetonius* says: *Cavebatur, ut in posterum simili modo ex ta Marti redderentur* (4). The horizontal stroke over the letter *N* appears on the stone, which is often omitted or defaced.

4. The person, who conducted this affair, is called *CAIVS SEVERIVS*; the former of which names occurs very frequently, and the latter several times, in *Gruter*. But the following word *EMERITVS*, tho it be

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(1) *Annal. Roman. Tom. III. p. 539.*

(2) *Cenotaph. Pisan. Dissert. I. pag. 52.*

(3) *Britan. Roman. Northumb. LXXX. Cumberl. LI, LII.*

(4) *In vit. August. cap. I.*

be found also as a proper name in *Gruter*, yet more usually signifies a *veteran soldier*. And therefore, as the inverted O, which comes after it, is the common character for CENTVRIO, a military officer in the Roman infantry, who commanded a hundred men ; it may possibly be doubted, whether by *emeritus centurio* might not here be meant a *superannuated captain*. Besides, it is well known, that the word *centurio* in other inscriptions is generally followed by the name of the legion or cohort, to which that officer belonged ; which is here wanting. And in *Gruter's* collection mention is made of one *Sergius Terentius, Sergii filius, Aemilianus, centurio emeritus* (1). But however, as in *Gruter* not only the word *emeritus* is put after *centurio*, but the person has likewise the *cognomen* of *Aemilianus* ; I am inclined to think *Emeritus* was the *cognomen* of this *Caius Severius*, and that the legion or cohort might be omitted for want of room to insert it on the stone.

5. The single imperfect word in the last line is neither by far so large, nor so deeply cut, as the rest of the inscription ; which, I presume, was occasioned also merely for want of room. In all the copies I have seen, it is written PEG. and Mr. *Hoare* tells me, that the last letter is certainly a G. However I cannot much doubt, but it was designed for an abbreviation of the word PECVNIA, and had once the pronoun SVA before it. For nothing was more common in public benefactions, than for the persons, who bore the expence of them, to express it by the

words

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(1) *Pag. DLXIII. num. 4.*

words *sua pecunia*, *sua impensa*, or *suo sumptu*, in the inscriptions prefixed to them, for the sake of perpetuating their own memory. There are, it seems, no traces of the word *sua* now remaining; but on the contrary, as Mr. Hoare says, the stone is *remarkably smooth* in that part: which makes me suspect, there has been an erasement; tho when, or for what reason it was made, would now be in vain to inquire at so great a distance of time. Nor is this a single instance of that kind. I shall therefore only observe further, that the letter G is no objection against the word *pecunia* being there intended. For as the two letters c and G had a great affinity in their sound, so they were often written one for the other. *Reinesius* has collected a large catalogue of both these changes from antient authorities. Among the latter sort, with which we are concerned, are *Progne* for *Procne*, *leg-tus* for *lectus*, *Tutigia* for *Tuticia*, and *carruga* for *carruca* (1); to which I shall only add *Gaesari* for *Caesari*, from another of our British inscriptions (2).

6. There are no points in the whole inscription, except one in the third line, after the word *insolentiam*; which may be supposed to have come there rather by chance, than design, as it can be of no significance.

7. After the *Romans* had abandoned this island, their monuments of all kinds were left as spoils to the inhabitants; who either destroyed or converted them to their own use, as they pleased. And many

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(1) *Reinesf. class. i. num. 2. p. 4.*

(2) *Brit. Rom. Northumb. LXXVII.*

of them have at different times been removed to a considerable distance from the places, where at first they stood. So two of those mentioned by *Camden*, as then fixed in the inner side of the wall at *Bath*, between the north and west gates (1), had doubtless been conveyed from the coemetery. And it may with equal reason be supposed, that this was likewise brought from thence.

But who the reigning emperor was, at the time this stone was set up, no intimation is given in the inscription. Tho, if one may be allowed to conjecture, the form of the letters suits very well with some others in the reign of *Severus*. And perhaps no time was more open for such licentious practices, as might justly merit the name *insolentia*, than the loose reign of *Commodus*; who, altho he was not the immediate predecessor of *Severus*, yet died but a few months before he came to the empire. Besides, we have two other inscriptions found in *Britain*, addressed *Numinibus Augustorum*; both which are thought to relate to *Severus* and his elder son *Caracalla*, after he was joined with his father in the government (2). Nor can there be any doubt of this, as to one of them at least; which is an altar, and has on one side of it the names of both his sons, *Caracalla* and *Geta*, as consuls that year. So that upon the whole, I can find no other period of time so probable for fixing the date of this inscription.

G. C. Nov. 2. 1753.

John Ward.

(1) *Britann.* pag. 172. edit. 1607.

(2) *Britann. Rom. Northumb.* LXXXVIII. *Yorksh.* XVIII.

LII. *A Letter to the right honourable the Earl of Macclesfield, President of the Royal Society, from Mr. Benjamin Wilfon, F.R.S. concerning some electrical Experiments, made at Paris.*

My Lord,

Sept. 18, 1753, Great Queen-str.  
London.

Read Dec. 6, 1753. ON the 7<sup>th</sup> of this month I was at the palace of St. German's near Paris, where I had the satisfaction of seeing Dr. le Monnier's experiment relating to the electricity of the air; of which an account was sent me by Abbè Mazeas, and was read before our honourable Society in December last; and I observed, that though the wind was westerly, and the air moist, yet the suspended wire, at different times, attracted very light bodies, at very small distances; the mean of which distances seem'd to be about  $\frac{1}{10}$  of an inch.

Abbè Mazeas informed me, that Dr. le Monnier, some months ago, had read a paper, at a meeting of the Royal Academy of Sciences, in which he told them, that he had great reason to believe the electric matter did not come from the *earth* at all, but from the *air*. Upon my mentioning this to the Doctor, I found him still of the same opinion.

As there was a convenient *apparatus* in his apartment, I propos'd making the experiments: for I always thought that the electric matter came from both, but principally from the earth; and that, probably, a difference of 10 to 1 would be perceived, upon making the experiments.

X x 2

The



The machine was suspended by silk lines, in such a manner, that every part of it was not less than two feet distant from any non-electric. The lines were dried by a chafing-dish of fire made with charcoal, as was likewise the glass globe; and every other precaution was strictly observed, that seemed necessary for making the experiments.

The Doctor appeared to be well versed in electrical inquiries, and shewed great judgment in conducting the whole. He got upon the suspended *apparatus* himself, and rubbed the globe with both his hands; whilst another person, who was likewise suspended, turned the wheel of the machine. Close to the globe was a slender slip of lead; at one end of which was fastened some brass tinsel, to serve as a collector of the electric matter. The other end of the lead had a communication with a tin tube, which was supported by silk lines about a foot in length: and as this tube hung higher than I could reach, another was hooked to it by means of a wire which hung down to a convenient distance.

As I stood upon the floor, I took hold of this last tube, whilst the glass was rubbed, that the *apparatus*, and the persons on it, might lose as much of their natural electricity as possible under such circumstances. On removing my hand, and afterwards approaching the tube, sometimes with my finger, and at other times with a key, we observed very small explosions, which were little more than just sensible.

I then desired one of the Doctor's servants (who likewise stood upon the floor) to lay hold of the suspended *apparatus* on which the Doctor was mounted, whilst the friction of the globe was continued. Im-  
mediately

mediatley on approaching the tube as before, with my finger, and then with the key, a very great difference was observed; for now the explosion was very large compared with the former trials. Doctor le Monnier desired the experiments might be repeated; which was done several times, and, to all appearance, the differences were the same.

He was perfectly satisfied that the experiments were fairly made, and that the explosion was much greater when the *apparatus* communicated with the *earth*, than when it communicated with the *air* only.

As several gentlemen of the Royal Academy of Sciences in Paris were of opinion that these experiments deserved attention, I thought your lordship would not be displeased, if I did myself the honour of communicating them.

I am,

My Lord,

Your Lordship's most obedient;

and obliged humble servant,

Benjamin Wilfon.

For a series of experiments and observations where the whole electrical apparatus was supported by electrics *per se*, see *Phil. Trans.* Vol. XLIV. p. 713—729—739, 740. See also Vol. XLV. p. 93—101 and Vol. XLVII. p. 371, 373. And Mr. Wilfon's *Essay*, and *Treatise, on Electricity*.

LIII. *Electrical Experiments, with an Attempt to account for their several Phænomena; together with some Observations on Thunder-Clouds, by John Canton, M. A. and F. R. S.*

*Experiment 1.*

Read Dec. 6,  
1753.

**F**ROM the cieling, or any convenient part of a room, let two cork-balls, each about the bigness of a small pea, be suspended by linen threads of eight or nine inches in length, so as to be in contact with each other. Bring the excited glass tube under the balls, and they will be separated by it, when held at the distance of three or four feet; let it be brought nearer, and they will stand farther apart; intirely withdraw it, and they will immediately come together. This experiment may be made with very small brass balls hung by silver wire; and will succeed as well with sealing-wax made electrical, as with glass.

*Experiment 2.*

If two cork-balls be suspended by dry silk threads, the excited tube must be brought within eighteen inches before they will repel each other; which they will continue to do, for some time, after the tube is taken away.

As the balls in the first experiment are not insulated, they cannot properly be said to be electrified: but when they hang within the atmosphere of the  
excited

excited tube, they may attract and condense the electrical fluid round about them, and be separated by the repulsion of its particles. It is conjectur'd also, that the balls at this time contain less than their common share of the electrical fluid, on account of the repelling power of that which surrounds them ; tho' some, perhaps, is continually entering and passing thro' the threads. And if that be the case, the reason is plain, why the balls hung by silk, in the second experiment, must be in a much more dense part of the atmosphere of the tube, before they will repel each other. At the approach of an excited stick of wax to the balls, in the first experiment, the electrical fire is supposed to come through the threads into the balls, and be condensed there, in its passage towards the wax : for, according to Mr. *Franklin*, excited glass *emits* the electrical fluid, but excited wax *receives* it.

### *Experiment 3.*

Let a tin tube, of four or five feet in length, and about two inches in diameter, be insulated by silk ; and from one end of it let the cork-balls be suspended by linen threads. Electrify it, by bringing the excited glass tube near the other end, so as that the balls may stand an inch and an half, or two inches apart : then, at the approach of the excited tube, they will by degrees lose their repelling power, and come into contact ; and as the tube is brought still nearer, they will separate again to as great a distance as before : in the return of the tube they will approach each other till they touch, and then repel as at first. If the tin-tube be electrified by wax, or the wire of a charged

charg'd phial, the balls will be affected in the same manner at the approach of excited wax, or the wire of the phial.

*Experiment 4.*

Electrify the cork-balls as in the last experiment by glass; and at the approach of an excited stick of wax their repulsion will be increased. The effect will be the same, if the excited glass be brought towards them, when they have been electrified by wax.

The bringing the excited glass to the end, or edge of the tin-tube, in the third experiment, is suppos'd to electrify it positively, or to add to the electrical fire it before contained; and therefore some will be running off through the balls, and they will repel each other. But at the approach of excited glass, which likewise *emits* the electrical fluid, the discharge of it from the balls will be diminish'd; or part will be driven back, by a force acting in a contrary direction; and they will come nearer together. If the tube be held at such a distance from the balls, that the excess of the density of the fluid round about them, above the common quantity in air, be equal to the excess of the density of that within them, above the common quantity contain'd in cork; their repulsion will be quite destroy'd. But if the tube be brought nearer; the fluid without, being more dense than that within the balls, it will be attracted by them, and they will recede from each other again.

When the apparatus has lost part of its natural share of this fluid, by the approach of excited wax to one end of it, or is electrified negatively; the electrical  
fire

fire is attracted and imbib'd by the balls to supply the deficiency; and that more plentifully at the approach of excited glass, or a body positively electrified, than before; whence the distance between the balls will be increased, as the fluid surrounding them is augmented. And in general, whether by the approach or recess of any body; if the difference between the density of the internal and external fluid be increased, or diminished; the repulsion of the balls will be increased, or diminished, accordingly.

*Experiment 5.*

When the insulated tin tube is not electrified, bring the excited glass tube towards the middle of it, so as to be nearly at right angles with it, and the balls at the end will repel each other; and the more so, as the excited tube is brought nearer. When it has been held a few seconds, at the distance of about six inches, withdraw it, and the balls will approach each other till they touch; and then separating again, as the tube is moved farther off, will continue to repel when it is taken quite away. And this repulsion between the balls will be increased by the approach of excited glass, but diminished by excited wax; just as if the apparatus had been electrified by wax, after the manner described in the third experiment.

*Experiment 6.*

Insulate two tin tubes, distinguished by *A* and *B*, so as to be in a line with each other, and about half an inch apart; and at the remote end of each, let a pair of cork balls be suspended. Towards the middle

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of *A*, bring the excited glass tube; and holding it a short time, at the distance of a few inches, each pair of balls will be observed to separate: withdraw the tube, and the balls of *A* will come together, and then repel each other again; but those of *B* will hardly be affected. By the approach of the excited glass tube, held under the balls of *A*, their repulsion will be increased: but if the tube be brought, in the same manner, towards the balls of *B*, their repulsion will be diminished.

In the fifth experiment, the common stock of electrical matter in the tin tube, is supposed to be attenuated about the middle, and to be condensed at the ends, by the repelling power of the atmosphere of the excited glass tube, when held near it. And perhaps the tin tube may lose some of its natural quantity of the electrical fluid, before it receives any from the glass; as that fluid will more readily run off from the ends or edges of it, than enter at the middle: and accordingly, when the glass tube is withdrawn, and the fluid is again equally diffused through the apparatus, it is found to be electrified negatively: For excited glass brought under the balls will increase their repulsion.

In the sixth experiment, part of the fluid driven out of one tin tube enters the other; which is found to be electrified positively, by the decreasing of the repulsion of its balls, at the approach of excited glass.

### *Experiment 7.*

Let the tin tube, with a pair of balls at one end, be placed three feet at least from any part of the room, and the air render'd very dry by means of a fire:

fire: electrify the apparatus to a considerable degree; then touch the tin tube with a finger, or any other conductor, and the balls will, notwithstanding, continue to repel each other; tho' not at so great a distance as before.

The air surrounding the apparatus to the distance of two or three feet, is supposed to contain more or less of the electrical fire, than its common share, as the tin tube is electrified positively, or negatively; and when very dry, may not part with its overplus, or have its deficiency supplied so suddenly, as the tin; but may continue to be electrified, after that has been touch'd, for a considerable time.

### *Experiment 8.*

Having made the Torricellian vacuum about five feet long, after the manner described in the *Philosophical Transactions*, Vol. xlvii. p. 370. if the excited tube be brought within a small distance of it, a light will be seen thro' more than half its length: which soon vanishes, if the tube be not brought nearer; but will appear again, as that is moved farther off. This may be repeated several times, without exciting the tube afresh.

This experiment may be consider'd as a kind of ocular demonstration of the truth of Mr. Franklin's hypothesis; that when the electrical fluid is condensed on one side of thin glass, it will be repelled from the other, if it meets with no resistance. According to which, at the approach of the excited tube, the fire is supposed to be repelled from the inside of the glass surrounding the vacuum, and to



be carried off thro' the columns of mercury ; but, as the tube is withdrawn, the fire is supposed to return.

*Experiment 9.*

Let an excited stick of wax, of two feet and an half in length, and about an inch in diameter, be held near its middle. Excite the glass tube, and draw it over one half of it; then, turning it a little about its axis, let the tube be excited again, and drawn over the same half; and let this operation be repeated several times: then will that half destroy the repelling power of balls electrified by glass, and the other half will increase it.

By this experiment it appears, that wax also may be electrified positively and negatively. And it is probable, that all bodies whatsoever may have the quantity they contain of the electrical fluid, increased, or diminished. The clouds, I have observed, by a great number of experiments, to be some in a positive, and others in a negative state of electricity. For the cork balls, electrified by them, will sometimes close at the approach of excited glass; and at other times be separated to a greater distance. And this change I have known to happen five or six times in less than half an hour; the balls coming together each time, and remaining in contact a few seconds, before they repel each other again. It may likewise easily be discover'd, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced into it by a positive one: and by whichsoever it be electrified, should that cloud either part with its overplus, or have its deficiency supplied suddenly, the apparatus will lose its electricity: which

is frequently observed to be the case, immediately after a flash of lightning. Yet when the air is very dry, the apparatus will continue to be electrified for ten minutes, or a quarter of an hour, after the clouds have passed the zenith; and sometimes till they appear more than half-way towards the horizon. Rain, especially when the drops are large, generally brings down the electrical fire: and hail, in summer, I believe never fails. When the apparatus was last electrified, it was by the fall of thawing snow; which happened so lately, as on the 12<sup>th</sup> of November; that being the twenty-sixth day, and sixty-first time, it has been electrified, since it was first set up; which was about the middle of May. And as Fahrenheit's thermometer was but seven degrees above freezing, it is supposed the winter will not intirely put a stop to observations of this sort. At London, no more than two thunder-storms have happened during the whole summer: and the apparatus was sometimes so strongly electrified in one of them, that the bells, which have been frequently rung by the clouds, so loud as to be heard in every room of the house (the doors being open), were silenced by the almost constant stream of dense electrical fire, between each bell and the brass ball, which would not suffer it to strike.

I shall conclude this paper, already too long, with the following queries:

1. May not air, suddenly rarefied, give electrical fire to, and air suddenly condensed, receive electrical fire from, clouds and vapours passing through it?

2. Is not the *aurora borealis*, the flashing of electrical fire from positive, towards negative clouds at a  
great

great distance, through the upper part of the atmosphere, where the resistance is least?

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LIV. *Extract of a Letter from Professor Bosc, of Wittemberg, to the Right Honourable George Earl of Macclesfield, Pr. R. S. with Observations thereupon, by Mr. Wm. Watson, F. R. S.*

Wittemberg, August 1, 1753.

Read Dec. 6, 1753. **I**N the beginning of August 1752, after great and continued rains, many of our rivers overflowed their banks, and covered the neighbouring grounds, more or less according to their level, to a considerable distance: and the quantity of water was so great, that in some places it was not discharged for more than a week. More particularly the river Unstrut in the territory of the landgrave of Thuringue required a very great time to empty itself, not only as that river runs over a large tract of country, but also as between Artern and great Jena, where this river joins the Sale, its bed in several places is very much confined.

When the inundation was abated, it was observed from the little city Laucha quite up above Artern, not only upon the fields and meadows, but also upon the bushes and trees, that there was a green and very tough viscous slime, which by the help of a stick could be drawn out to two or three ells in length. The subsequent heat of the sun dried this matter, and it appeared like wool upon the bushes; but the fields,  
when

when seen at a distance, seemed as though they were covered with sand. This matter had a smooth appearance outwards, but within was like a sheep's-skin. Downwards next the ground it had a sort of wool; and when the whole was washed with soap, it whitened, and appeared like a clean fleece of white wool. Of this substance the country-people soon made wicks for their lamps, as several lined their cloaths with it, as they would with furs.

It was further observed, that where this substance was mowed off from the meadows, the grass under it was quickly dried up; but, where it was not removed, the grass in the following December was as green and fresh as in the spring. — Thus far Mr. Bose.

### Observations.

The vegetable substance, which, upon the specimen sent over by the professor to our truly Noble and Learned President, he has intituled “a sort, “perhaps, of *Alcyonium molle*,” is a species of that genus of plants, which the more modern botanists call *byssus*. And it is of that species, or a very slight variety therefrom, which is called, by the justly celebrated Dillenius \*, in his *Historia Muscorum*, *Byssus tenerrima viridis velutum referens*. It is also mentioned and figured by Micheli † in his *Nova Plantarum Genera*, under the title of *Byssus terrestris viridis herbacea et mollissima, filamentis ramosis et non ramosis*. This genus of plants, in the order of nature, comes between the mosses and  
*fungi*.

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\* *Histor. Muscor.* p. 7.

† *Michelii Nov. Plant. Genera*, p. 211. Tab. LXXXIX. Fig. 5.

*fungi*. The specimen now sent, being white on one side, arises from its either being washed or bleached by the sun; for when wet, according to Mr. Bose, it was green: and this colour is mentioned both by Dillenius and Micheli in their several denominations.

This vegetable is found in England, as well as in many parts of Europe, in moist meadows, covering the ground like a carpet, and sometimes to a great extent.

We must be careful, however, how we connect the substance in question, and others of the same genus with the βύσσος of the antient Greek writers, or the *byssus* of the Latin. What that substance was, has been matter of great controversy. Thus much is certain, that garments made of it were the apparel of the rich. And in the New Testament, St. Luke, in the parable of the rich man and Lazarus, says of the former, as a mark of his opulence, ἐνεδιδύσκετο πορφύραν καὶ βύσσον; this is translated in our English version, "he was cloathed in purple and fine linen". It is more probable, that the *byssus* of the ancients was a very fine sort of cotton: but whoever has the curiosity of examining what has been said upon this subject, may consult Fliny \* and Wormius †; but, above all, Bodæus à Stapel §, in his commentary upon Theophrastus; who has, upon this occasion, as well as upon a great variety of others, given us an ample testimony of his vast erudition.

\* *Plinii* lib. xix. c. i.  
 § *Pag.* 425, et seq.

† *Mus.* p. 139.

LV. *An Account of a Memoir read at the Royal Academy of Sciences at Paris, by M. de Barros, a Portuguese Gentleman, concerning certain Phænomena observed by him at Paris, in the last Transit of Mercury over the Sun: by J. Short, A.M. F.R.S.*

Read Dec. 13, 1753. **T**HE author says, he made use of an excellent Gregorian reflector of four feet in length, taking in the eye-piece, together with as much of the great tube, as exceeds the focal lengths of the two eye-glasses (*I suppose it should be the two speculums*): that the focus of the great speculum is 33 (*Paris*) inches; that of the small one four inches; the focus of the eye-glass next the eye 18 lines; the focus of the glass furthest from the eye 5 inches; and, lastly, that the combined power of these two glasses is nearly equal to that of a single eye-glass of 3 inches. This telescope, therefore, according to my computation, magnified about 130 times.

That he was placed in the most commodious situation for observing the egress; that his smoaked glass was fixed perpendicular to the axis of his telescope within a close tube; and that he always made use of the same part of this glass.

He took notice, that the interior contact of Mercury's and the Sun's limbs \* was very rapid, having observed it with a green-colour'd glass held over the

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smoaked

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\* At 10<sup>h</sup> 18' 41".

fmoaked glafs: immediately after which, looking thro' the fmoaked glafs only, he perceived, that a fmall thread of light was ftill vifible between the limbs, before, what he calls, the fecond contact took place, which was not till four feconds after; that the exterior contact appeared ftationary, or feemed to laft 6 or 7 feconds; that having obferved the total egress with the coloured glafs upon the fmoaked one, he brought Mercury upon the Sun's limb again, by removing the colour'd glafs; and that the fecond total egress did not happen till 6 or 7 feconds after the firft. When he obferved him at the diftance of about 3 of his diameters from the Sun's limb with both the glaffes, he remarked, that the faid diftance feem'd diminished, and Mercury's diameter increafed. That the part of the Sun's limb, where Mercury went off, to the extent of fix degrees of circumferencce, feemed under much the fame configuration, as the illuminate limb of the Moon about the quadrature, fomewhat uneven and undulating. The fame looked alfo redder than the reft of the difk. This was about 18 or 20 feconds before Mercury difappeared, and was feen thro' the fmoaked glafs alone: for when the green glafs was applied, the appearance in a manner vanished.

The evening before the tranfit, he viewed the Sun with different-colour'd glaffes, variously combined with one another, and with a fmoaked glafs; and found, that a green glafs before the fmoaked one did beft: the Sun appearing of a filvery huc, like the Moon, and the fpoats and the limb exceedingly well defined.

M. de Barros, having thus defcribed the particular phenomena, attempts, and indeed very ingenioufly, to

account for them all, from this single supposition; namely, that the disks of the Sun, and of Mercury seen thereon, are environed with a certain *corona* of light (like that which Sir Isaac Newton calls the circle of *aberration* or diffipation in refracting telescopes), whereby the apparent diameter of the Sun is enlarged, and that of Mercury contracted.

But, as this gentleman made use of a reflecting telescope, and as no such circle, from the known principle of reflexion, can take place in such a telescope, if well made, as Sir Isaac has proved long ago; I shall not take up the Society's time in pursuing him thro' all his particular suppositions; but shall endeavour to shew, that his hypothesis has really no foundation.

Sir Isaac, as I before hinted, in several of his writings, remarks, that the images of all objects seen in refracting telescopes are surrounded with a circle of aberration; which is always less, the longer the telescopes are. In his optics he tells us, that, to avoid the indistinctness arising from this circle, he would propose catadioptric telescopes, in which, if the speculums, under limited apertures, be justly figured, no such circle of aberration can confuse the image: but, if the speculums are of a spherical figure, with too large apertures, then indeed a circle of aberration will take place; as it also will, when the figure deviates from the circular towards the hyperbolic, even under a small aperture; and the same thing will happen, if the spherical figure be inaccurate.

About three days from the change of the Moon her whole body is visible: that part of the limb, which is directly enlightened by the solar rays ap-



pearing to the naked eye, as an arc of a greater circle, than the other, which receives the reflex light from the earth. Look thro' a refracting telescope, and you will perceive the apparent difference of these circles very much diminished: and if they be view'd with a good reflector, they will be perfectly reduced to an equality, even if measured with a micrometer in the focus, as we have often found.

If a reflecting telescope, well constructed in all respects, be directed any considerable time to the sun, such a circle of aberration will be generated, from the little speculum's being heated, and thereby its figure alter'd, from the sun's rays falling condensed thereon from the great one; and if it continues long under this circumstance, the image will be render'd utterly indistinct and confused.

This we were thoroughly convinced of at the above-mention'd transit of Mercury: for the reflector, a very good one, which we used, in taking, with the the micrometer, the differences of right ascension and declination between the planet and the sun's limb, having been a good while exposed to the direct rays, was found at last to give a very indistinct image; but was restored to its former degree of perfection, by turning it from the sun, and screwing off the eye-piece, so as to admit the cool air into the great tube, whereby the over-heated small speculum soon recovered its due temper and figure. The last-mentioned effect is scarce sensible in the lesser reflectors of small apertures; but in those of large ones it is very considerable.

Dr. Bevis, Mr. Canton, and Mr. Bird, who viewed Mercury going off the sun, with very good reflectors of different lengths, assure me, they saw him quite  
distinct

distinct and free from any *corona*, or circle of aberration, and the sun's limb perfectly well defined. For my own part, he appeared to me, thro' a reflector of four feet focus, magnifying about 135 times, as truly defined as I could wish to see a black circle upon a white ground.

I must, however, upon this occasion, take notice, that, during the whole time of this transit of Mercury, the air was perfectly calm with us; but that, in the two last transits of Mercury over the sun, *viz.* in the years 1736 and 1743, both the sun's and Mercury's limbs appeared to me indistinct, and surrounded with something like what this gentleman calls a luminous crown, or circle of aberration; tho' I, at both these times, made use of reflecting telescopes, which I had, by former trials, esteemed good. But it is to be observed, that, during both these transits, there was a constant hard gale of wind; and as I had, by other observations, formerly found, that the images of the planets, in the night-time, did not appear so distinct in windy weather, as when it was calm, I therefore imputed the indistinctness of the sun's and Mercury's limbs to the air's being agitated by the wind\*. And of this we may be made sensible by a familiar instance:

Suppose a vessel full of water, having any thing lying at the bottom, such as a shilling, the water being at rest; you will then perceive the image of the shilling distinctly; but if you give any commotion to the water, the image of the shilling will then appear indistinct and confused.

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\* Since this paper was read, Mr. Short has been informed by M. Le Monnier, the French King's astronomer, that, during the last transit at Paris, they had a hard gale of wind from the N. E.

Somewhat analogous to this is this other appearance: If you look thro' a telescope at any of the planets, when the stars appear hazy, dim, and languid, you will see them distinctly: but look at them again, when the stars appear most bright and sparkling, you will then find their images less distinct. This may be accounted for by the just-mention'd instance of the vessel of water, by supposing air instead of water. And if we consider the infinite number of heterogeneous particles which continually float in the air, and suppose these to be at rest, or put into motion, we shall find, that it is not at all surprising, that we see the images of objects, placed beyond the medium of air, more or less distinct.

We are not so sensible of this indistinctness, arising from the agitation of the air, in refracting telescopes, as in reflectors: because the errors of reflexion, caused by any irregularity in their figure, or confusion in the air, are about five or six times greater than the same errors in refraction; even tho' both telescopes magnify the same number of times; as has long ago been demonstrated.

We also took notice of M. de Barros's first phenomenon; to wit, the seeming greater velocity of Mercury when he was near the egress: which we thus accounted for. When he was at a considerable distance from the limb, there being nothing near enough to refer his velocity to, he seemed in a manner stationary; but being advanced near the sun's edge, we could refer his motion to that, with ease; which thus becoming sensible, it might be esteemed rapid, in comparison of the former. I have often made the same remark on the gradual approach of two lumi-

nous

nous bodies, as the appulse of the moon's lucid limb to a star or planet.

The expedition with which the author observed his second phænomenon, is truly somewhat extraordinary; namely, that he should first observe what he names the final contact; secondly, that he should take away his green glass; and then, thirdly, that he should be able suddenly to alter the conformation of his eye, so as to see distinctly with a much greater influx of light, and then take another observation, and all in the short space of four seconds! Messieurs Mairan and Bouguer take notice of this in their certificate, which follows this memoir; and own, that his observations were such, as seem to have required the attention of several persons.

Upon the whole, we may conclude, that the several phænomena, observed by this gentleman, in the transit and egress of Mercury, were owing to indistinctness of vision, arising either from the eye, the telescope, or the air; and that this alone may account for them all, without having recourse to supposed circles of aberration; which can never possibly exist in a well-constructed reflecting telescope.

LVI. *An Explication of an obscure Passage in Albert Girard's Commentary upon Simon Stevin's Works (Vide Les Oeuvres Mathem. de Simon Stevin, à Leyde, 1634, p. 169, 170); by Mr. Simson, Professor of Mathematics at the University of Glasgow: Communicated by the Right Honourable Philip Earl Stanhope.*

Read Dec. 20, 1753. “ PUIS que je suis entré en la ma-  
 “ tiere des nombres rationaux,  
 “ j'adjoufteray encore deux ou trois particularitez,  
 “ non encon par cy devant practiquées, comme d'ex-  
 “ pliquer les radicaux extremement pres, &c.”

The first thing Albert Girard gives in this place is a method of expressing the ratio of the segments of a line cut in extreme and mean proportion, by rational numbers, that converge to the true ratio. For this purpose he takes the progression 0, 1, 1, 2, 3, 5, 8, 13, 21, &c. every term of which is equal to the sum of the two terms that precede it: and says, any number in this progression has unto the following the same ratio [nearly] that any other has to that, which follows it. Thus 5 has to 8 nearly the same ratio, that 8 has to 13; consequently, any 3 numbers next one another as 8, 13, 21, nearly express the segments of a line cut in extreme and mean proportion, and the whole line; so that 13, 21, 21, (*N. B.* 13 is wrong printed for the second number, instead of 21) constitute

stitute near enough an isosceles triangle, having the angle of a pentagon; *i. e.* whose angle at the vertex is subtended by the side of a pentagon in the circle described about the triangle.

Now this will be plain, if it be shewn, that the squares of the numbers in this series are alternately lesser and greater by an unit, than the product of the two numbers next them upon each side. Thus, in the four numbers, 5, 8, 13, 21, the square of 8 is an unit lesser than the product of 5 and 13; but the square of 13 that next follows 8, *viz.* 169, is an unit greater than 8 times 21, or 168; and so on constantly.

*Case 1.*

If  $a, b, c$ , be such numbers, that  $\begin{cases} 1. a + b = c \\ 2. ac = bb + 1 \end{cases}$

Then, if  $d$  be taken, so that  $d = b + c$ ; then shall  $bd + 1 = cc$ .

Because  $d = b + c$ ;  $bd + 1$  shall be  $= bb + bc + 1 = ac + bc$  [2] which is  $= a + b \times c = cc$  [1]: *Ergo*  $bd + 1 = cc$ .

*Case 2.*

If  $a, b, c$ , be such that  $\begin{cases} 1. a + b = c \\ 2. ac + 1 = bb \end{cases}$

Then, if  $d$  be taken, so that  $d = b + c$ ; then shall  $bd = cc + 1$ .

Because  $bd = bb + bc = ac + bc + 1$  [2.]  $= a + b \times c + 1 = cc + 1$  [1.]

*Problem.*

Having given the number  $a$ , in *Case 1.* to find  $b$  and  $c$ , *i. e.* having given  $a$  to find  $b$  such that  $bb + 1 = (ac =) aa + ab$ ; then is  $bb - ab = aa - 1$

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anc

and therefore  $b = \frac{a + \sqrt{5aa-4}}{2}$ . Whence, to make

$b$  a rational integer number,  $5aa-4$  must be a square; which it will be, if  $a=1$ ; and then  $b$  will also be 1, and  $c$  will be 2: and having continued the series, every number will have the properties mentioned.

The second thing which Albert Girard mentions, is a way of exhibiting a series of rational fractions, that converge to the square root of any number proposed, and that very fast. He tells nothing about the way of forming it, and only gives the two following examples; *viz.*

He says,  $\sqrt{2}$  is equal nearly to  $\frac{577}{408}$ : or, if you would have it nearer, to  $\frac{1393}{985}$ .

His other example is of  $\sqrt{10}$ , which, he says, is nearly equal to  $3\frac{53353}{328776}$ ; *i. e.* to  $\frac{1039681}{328776}$ . And these are the fractions your lordship has turned, at first sight, into continued fractions of the same value\*.

The way of making a series of rational fractions, which converge to the square root of any number proposed, in such a manner, that the square of the numerator of any of them being lessened by an unit, or, in some cases, increased by an unit, the remainder, or sum divided by the square of the denominator, shall be exactly equal to the number proposed, depends upon the following propositions:

*Prop.*

\* N. B. That the continued fraction here alluded to for expressing the square root of 10 was  $\frac{1}{2} \times 19 - \frac{1}{2}$

$-\frac{1}{32}$

$-\frac{1}{578}$

$-\frac{1}{3125}, \text{ \&c. ad infinitum.}$

*Prop. 1.*

Let  $a$  be any number proposed, and  $\frac{b}{c}$  be such a fraction, that  $\frac{bb-1}{cc} = a$ , i. e.  $bb = acc + 1$ , then, if two other fractions be taken, one of which is  $\frac{b}{ac}$ , the first divided by the proposed number  $a$ , and the other is  $\frac{c}{b}$ , the reciprocal of the first fraction; then the fraction  $\frac{bb+acc}{2bc}$ , whose numerator is the sum of the products of the numerators, and of the denominators of the fractions  $\frac{b}{c}$  and  $\frac{b}{ac}$ ; and its denominator the sum of the products of the numerators, and of the denominators of the fractions  $\frac{b}{c}$  and  $\frac{c}{b}$ , shall have the same property with the fraction  $\frac{b}{c}$  i. e.  $\frac{bb+acc^2}{2bc^2} - 1 = a$ .

Because  $bb = acc + 1$

$bb - acc = 1$ , and squaring

$$b^4 - 2ab^2c^2 + a^2c^4 = 1. \text{ And adding } 4ab^2c^2$$

$$b^4 + 2ab^2c^2 + a^2c^4 = 4ab^2c^2 + 1.$$

Whence  $\frac{b^4 + 2ab^2c^2 + a^2c^4 - 1}{2bc^2} = a$ .

*Prop. 2.*

If  $\frac{b}{c}$  be such a fraction, that  $\frac{bb+1}{cc} = a$ , i. e.  $bb + 1 = acc$ , all other things remaining as in *Prop. 1.*; then shall the fraction  $\frac{bb+acc}{2bc}$ , formed as there described, be such, that  $\frac{bb+acc^2}{2bc^2} - 1 = a$ ,

A a a 2

Because



Beaufe  $bb+1=acc$ , then  $acc-bb=1$ ; and squaring  
 $b^4-2ab^2c^2+a^2c^4=1$ .

Whence, as in the foregoing, it will follow, that

$$\frac{bb+acc-1}{2bc^2} = a.$$

*Prop. 3.*

Let the fraction  $\frac{b}{c}$  be such, that  $\frac{bb-1}{cc} = a$ , i. e.  $bb=acc+1$ ; also let  $\frac{d}{e}$  be another fraction, having the same property with  $\frac{b}{c}$ , i. e. such, that  $dd=aee+1$ . Then, if from the fraction  $\frac{d}{e}$ , and the two others mentioned in *Prop. 1.* viz.  $\frac{b}{ac}$ , and  $\frac{c}{b}$ , a new fraction be formed, in the same manner as the fraction  $\frac{bb+acc}{2bc}$  was formed from  $\frac{b}{c}$ , and the same two  $\frac{b}{ac}$  and  $\frac{c}{b}$ , which fraction will be  $\frac{bd+ace}{cd+be}$ ; this new fraction shall have the same property with the other two  $\frac{b}{c}$  and  $\frac{d}{e}$ , i. e.

$$\frac{bd+ace-1}{cd+be} = a.$$

1.  $bb=acc+1$

2.  $dd=aee+1$

3.  $ac^2d^2=a^2c^2e^2+ac^2$  [2.]

4.  $b^2d^2=ab^2e^2+b^2$  [2.]

5.  $b^2d^2=ab^2e^2+ac^2+1$  [4, 1.]

6.  $b^2d^2+a^2c^2e^2=ab^2e^2+a^2c^2e^2+ac^2+1$  [5.]

7.  $b^2d^2+a^2c^2e^2=ac^2d^2+ab^2e^2+1$  [6. 3]

8.  $b^2d^2+2abcde+a^2c^2e^2=ac^2d^2+2abcde+ab^2e^2+1$  [7].

$$i. e. \overline{bd+ace}^2 = a \times \overline{cd+be}^2 + 1.$$

$$9. \frac{\overline{bd+ace}^2 - 1}{\overline{cd+be}^2} = a.$$

*Prop. 4.*

The same things being supposed as in *Prop. 3.* except that  $bb$ , instead of being equal to  $acc+1$ , as there, is equal to  $acc-1$ , or  $bb+1=acc$ ; it will follow, by the like steps as in *Proposition 3.* that

$$\frac{\overline{bd+ace}^2 + 1}{\overline{cd+be}^2} = a.$$

*Prop. 5.*

If likewise  $d^2$  be equal to  $ace-1$ , as well as  $b^2 = acc-1$ , all other things remaining as in *Proposition 3.* then shall  $\overline{bd+ace}^2 = a \times \overline{cd+be}^2 + 1$ , i. e.

$$\frac{\overline{bd+ace}^2 - 1}{\overline{cd+be}^2} = a.$$

- $$\left\{ \begin{array}{l} 1. b^2+1=acc \\ 2. d^2+1=ace \\ 3. b^2d^2+b^2=ab^2e^2 [2.] \\ 4. ac^2d^2+ac^2=a^2c^2e^2 [2.] \\ 5. b^2d^2+ac^2=ab^2e^2+1 [3, 1.] \\ 6. b^2d^2+ac^2+ac^2d^2=ac^2d^2+ab^2e^2+1 [5.] \\ 7. b^2d^2+a^2c^2e^2=ac^2d^2+ab^2e^2+1 [6.4.] \\ 8. b^2d^2+2abcde+a^2c^2e^2=ac^2d^2+2abcde+ab^2e^2+1 [7.] \end{array} \right.$$

$$i. e. \overline{bd+ace}^2 = a \times \overline{cd+be}^2 + 1.$$

$$9. \frac{\overline{bd+ace}^2 - 1}{\overline{cd+be}^2} = a.$$

## Prop. 6.

But if  $b^2 = acc + 1$ , and  $d^2 = ace - 1$ , all other things remaining as in *Prop. 3*. Then shall  $\overline{bd + ace}^2 + 1 = a \times \overline{cd + be}^2$ . i. e.  $\frac{\overline{bd + ace}^2 + 1}{\overline{cd + be}^2} = a$ . which may be shewn, as the rest were.

Now, let  $a$  be any number proposed, and let the fraction  $\frac{b}{c}$  be such, that either  $\frac{bb - 1}{cc} = a$ , or  $\frac{bb + 1}{cc} = a$ , and take the fractions  $\frac{b}{ac}$  and  $\frac{c}{b}$ , before described; then the series of fractions converging to  $\sqrt{a}$  will be as follows:

$\frac{c}{b}, \frac{b}{ac} \left\{ \frac{b}{c} = \text{the first term of the series.} \right.$

$\frac{bb + ace}{2bc} = \frac{d}{e}$  the second term. } Every term is formed from the preceding; and the 2 fractions  $\frac{b}{ac}$  and  $\frac{c}{b}$ , in the same manner as the second from the first, and these fractions.

$\frac{bd + ace}{cd + be} = \frac{f}{g}$  the third term.

$\frac{bf + ace}{cf + bg} = \frac{h}{k}$  the fourth term.

&c. in infin.

And from the foregoing propositions it follows,

1. That if  $\frac{bb - 1}{cc} = a$ , then every fraction of the series shall be such.

That if from the square of its numerator be taken an unit, the remainder, divided by the square of its denominator, shall be equal to  $a$ .

For, by *Prop. 1*. the fraction  $\frac{d}{e}$  shall be such; and by *Prop. 3*. the next fraction  $\frac{f}{g}$  shall likewise be such; and so all the following terms.

*Example.*

*Example.*

Let  $a=2$  ; then the first fraction, *i. e.* that in the smallest numbers,  $\frac{b}{c}$ , that makes  $\frac{bb-1}{cc}=2$ , is when  $b=3$ , and  $c=2$  ; so that

$$\frac{c}{b} \cdot \frac{b}{ac} \left\} \frac{b}{c}$$

are      And the terms following the first  $\frac{3}{2}$

$$\frac{2}{3} \cdot \frac{3}{4} \left\} \frac{3}{2} \text{ are } \frac{17}{12} \cdot \frac{99}{70} \cdot \frac{577}{408} \cdot \frac{3363}{2378} \cdot \&c.$$

2. But if  $\frac{bb+1}{cc}=a$ , *i. e.* if the first fraction  $\frac{b}{c}$  of the series have the square of its numerator an unit less than  $acc$ , the multiple of the square of its denominator by the number  $a$  ; the second term shall have the square of its numerator an unit greater than the said multiple of the square of its denominator ; and the third term shall have the said square an unit lesser, and so on alternately.

For, by *Prop. 2.* the second term  $\frac{d}{e}$  shall be such, that  $\frac{dd-1}{ee}=a$  : and therefore, by *Prop. 4.* the third term  $\frac{f}{g}$  shall be such, that  $\frac{ff+1}{cc}=a$ . And by *Prop. 5.* it follows, that the next term  $\frac{h}{k}$  shall be such, that  $\frac{hh-1}{kk}=a$  ; and so on alternately, by *Prop. 4.* and 5.

*Example.*

Let  $a=2$  ; then the first fraction  $\frac{b}{c}$  that makes  $\frac{bb+1}{cc}=2$ , is when  $b=1$ , and  $c=1$ . So that

$$\left. \begin{array}{l} c \\ b \end{array} \cdot \frac{ac}{b} \right\} \frac{b}{c}$$

And the following terms

$$\text{are } \left. \begin{array}{l} \frac{1}{1} \cdot \frac{1}{2} \end{array} \right\} \frac{1}{1} \text{ are } \frac{1}{2}, \frac{7}{3}, \frac{17}{4}, \frac{41}{5}, \frac{99}{6} \text{ \&c.}$$

But if  $a$  be 13, then the first fraction will be  $\frac{13}{2}$   
 $\frac{13}{2} \cdot \frac{13}{6} \left\} \frac{13}{3}, \frac{649}{180}, \frac{23382}{6480} \text{ \&c.}$

3. But if the fraction  $\frac{b}{c}$  be such, that  $\frac{bb-1}{cc}=a$ , and if the fractions  $\frac{b}{ac}, \frac{c}{b}$ , be taken, from which the series is to be formed, as has been described; then, if the first fraction of the series be made not  $\frac{b}{c}$ , but some fraction  $\frac{d}{e}$ , such that  $\frac{dd+1}{ee}=a$ ; then shall every term of the series be such as the fraction  $\frac{d}{e}$ , *i. e.* the square of the numerator being increased by an unit, and the sum divided by the square of the denominator, the quotient shall be equal to  $a$ .

For, since  $bb=acc+1$ , and  $dd=aea-1$ , by *Prop. 6.* it follows, that the next term  $\frac{f}{g}$  shall be such, that  $\frac{ff+1}{gg}=a$ ; and so on for every term.

*Example.*

Let  $a=2$   $\frac{b}{c}=\frac{3}{2}$ ; then will  $\frac{b}{ac}=\frac{3}{4}$ , and  $\frac{c}{b}=\frac{2}{3}$ , and let  $\frac{d}{e}=\frac{1}{1}$ ; then  
 $\left. \begin{array}{l} c \\ b \end{array} \cdot \frac{b}{ac} \right\} \frac{d}{e}$   
 $\frac{1}{1} \cdot \frac{1}{1} \left\} \frac{1}{1}$   
 $\frac{1}{2} \cdot \frac{1}{3} \left\} \frac{1}{2}$

are 7.  $\frac{41}{5}$ .  $\frac{239}{29}$ .  $\frac{1393}{169}$ .  $\frac{1393}{985}$ . &c.

To find  $\frac{b}{c}$  such as makes  $bb - 1 = acc$ , i. e.  $acc + 1 = bb$ , recourse must be had to Lord Brouncker's method in Dr. Wallis's *Commercium Epistolicum*.

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LVII. *Observations upon the Electricity of the Air, made at the Chateau de Maintenon, during the Months of June, July, and October, 1753; being Part of a Letter from the Abbé Mazeas, F.R.S. to the Rev. Stephen Hales, D. D. F. R. S. Translated from the French by James Parsons, M. D. F. R. S.*

S I R,

Read Dec. 20, 1753. **B**EING assured, that the electricity of the atmosphere would yet afford means of entertaining you, I spent part of this summer in observing what nature presented me upon so important a subject.

On the 14th of June I accompanied the Marechal de Noailles to his castle of Maintenon. At my arrival, I set up an apparatus, which consisted of an iron wire 370 feet long, raised to 90 feet above the horizon. It came down from a very high room in the castle, where it was fastened to a silken cord six

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feet.

feet long, and was carried from thence to the steeple of the town ; where it was likewise fasten'd to another filken cord of eight feet long, and shelter'd from rain : and a large key was suspended by the end of this wire, in order to receive the electrical fluid.

*Observation 1.*

From the 17th of June, the time of beginning my experiments, the electricity of the air was sensibly felt every day, from sun-rise, to 7 or 8 in the evening ; except in moist weather, when I could perceive no signs of electricity. In dry weather, the wire attracted minute bodies, at no greater distance than three or four lines. I repeated the experiment carefully every day, and constantly observed, that, in weather void of storms, the electricity of a piece of sealing-wax of two inches long was above twice as strong as that of the air. This observation inclines me to conclude, that, in weather of equal dryness, the electricity of the air is always equal.

*Obs. 2.*

When I grasped the wire closely in my hand, the electricity ceased instantly, and did not recover till three or four minutes after : whereas, during a storm, we could deprive the wire of its electricity but a moment ; for it immediately return'd with the same vigour. Whence it appears, that the common electricity of the air has but a slow motion.

*Obs. 3.*

I endeavoured to increase the electricity of my wire, by the addition of a second, which communicated  
with

with an electrical magazine, composed of pieces of iron, tin plates, gilt paper, and such-like, sustained by filken cords; and I observed, 1. That the electrical fluid did not even then act with any more strength upon minute bodies presented to the wire. 2. That, in depriving this magazine of its electricity, it seem'd to return the more slowly, the more considerable the magazine was; whereas the contrary happens during a storm. This slowness, with which the common electricity of the air is propagated, makes me despair of finding means capable of rendering its motion sensible.

*Obs. 4.*

It does not appear to me, that hurricanes and tempests increase the electricity of the air, when they are not accompanied with thunder: for, during three days of a very violent continual wind in the month of July, we were obliged to put the dust within four or five lines of the conductor, before any sensible attraction could be perceived. The direction of the winds, whether east, west, north, or south, does not make any sensible alteration in the electricity of the air, except when they are moist. In the most dry nights of this summer, I could observe no signs of electricity in the air; but it returned in the morning, as I have said, when the sun began to appear above the horizon, and vanished again in the evening, about half an hour after sun-set. The strongest common electricity of the atmosphere, during this summer, was perceived in the month of July, on a very dry day, the heavens being very clear, and the sun extremely hot. The distance of ten or twelve lines



was then sufficient for the approach of the dust to the conductor, in order to see the particles rise in a vertical direction, like the filings of iron on the application of a magnet.

*Obs. 5.*

On the 27th of June, at 2 afternoon, I perceived some stormy clouds rising above the horizon, and immediately went up to my apparatus; and having applied the dust to the key, it was attracted with a force, which increased in proportion as the clouds reached the zenith. When they had come nearly over the wire, the dust was so impetuously repell'd, as to be intirely scatter'd from the paper. I drew considerable sparks from it, altho' there was neither thunder nor lightning. These sparks were of a very lively red colour, when I attracted them with my finger: they were white and smaller, when I used a wire fasted in a glass tube: they were bluish, and much extended, when attracted by spirit of wine in a silver spoon.

*Obs. 6.*

I applied a piece of resin to the conductor, but could draw no sparks from it: however, all, who were present, heard a noise like that of hairs when burnt. It was the same with sealing-wax, woollen-cloth, linen, &c. Then I took a quicksilver'd glass, and applied to the clean side a piece of wire of six inches long, whilst the other end was put to the conductor; by which I drew a multitude of small whitish sparks, which soon ceased, but were succeeded by a  
noise

noise, like that which happen'd upon applying the resin to the conductor.

I imagine, that this noise proceeds from the violent efforts made by the electrical fluid, in order to penetrate into the wire. Since that fluid has only a determined space to occupy in it, it is natural to think, that when that space is fill'd, the fluid ought to produce a noise like that, which is heard, when the bottle in the Leyden experiment is greatly charged.

When I applied the end of the wire to the silver'd surface of the glass, whilst the other end touched the conductor, the quicksilver affected me so strongly, that, notwithstanding my being so much accustomed to suffer these electrical shocks, I was not able to bear this.

From hence I conclude, that the best method of increasing the electrical power is to make it fall upon some metalline surface, intimately connected with a surface, that is an electric *per se*. And in order to bring reason and experience together, this is the manner, in which I think the power augmented :

When the electrical fluid is pushed with rapidity along the conductor (as it happens during a thunderstorm), it ought to be instantly diffused over the surface of the quicksilver. This fluid communicating with the glass more easily than with the air (which we shall prove by-and-by), it ought partly to enter into the substance of the glass: for the difficulty, which it finds in passing through its pores, gives it time to accumulate, and consequently to shock bodies applied to it with the greater power. But this method, which seems so convenient to increase the force of  
the

the electrical fluid in a storm, never succeeded for the common electricity of the air. I always found it nearly extinct, never exerting itself beyond a certain sphere of action, and that very inconsiderable.

*Obs. 7.*

When the stormy clouds were in the zenith of my wire, I observed, that the electricity was increased to so high a point, that the filken thread attracted light bodies at the distance of seven or eight inches. This cord was six feet long, and in the first foot the electricity was nearly as strong as in the wire, but from thence it diminished in the rest of the length. I substituted a glass tube to the filken cord, and observed the same phenomenon, with this difference, that the electrical fluid penetrated it with greater difficulty.

*Obs. 8.*

The stormy clouds, which I mentioned before, remained about two hours above the horizon, without either thunder or lightning; nor did a very heavy rain diminish the electricity, except about the end, when the clouds began to be dissipated; and for that time I left my apparatus.

About six o' clock in the evening I was told, that there were signs of a new storm in the air: I went up, and while I was preparing matters, a young man of the town, thirty-five years old, subject to an epilepsy, was among the spectators. The small time, which the storm lasted, was not enough to make many trials in; and the following was what I most attended to:

I drew

I drew sparks upon the epileptic person, who was present, from the first thunder-clap. At first he bore them; but in two or three minutes I perceived his countenance change; and, for fear that any accident should happen to him, I begg'd he would retire. He was no sooner returned home, than his senses fail'd him, and he was seized with a most violent fit. His convulsions were taken off with spirit of hartshorn; but his reason did not return in an hour and half. He went up-and-down-stairs, like one who walks in his sleep, without speaking or knowing any person, settling his papers, taking snuff, and offering chairs to all that came in. When he was spoken to, he pronounced inarticulate words, which had no connexion of sense.

When this poor man recovered his reason, he fell into another fit. His friends told me, that he was more affected with this distemper when it thunder'd, than at any other time; and that if at any time it happen'd, which it rarely did, that he then escaped, his eyes, his countenance, and the confusion of his expressions, sufficiently demonstrated the weakness of his reason.

The next day I learned from himself, that the fear of thunder was not the cause of his disease; but that, however, he found a fatal connexion between phenomenon and that distemper, with which Providence was pleased to afflict him. He added, that when the fit seized him, he perceived a vapour rising in his breast, with so much rapidity, that he lost all his senses, before he could call for help.

Such are the observations, Sir, which my moment of leisure suffer'd me to make: I should have been

very glad, if the matter were more thoroughly examined, and my researches more worthy of being presented to you: but you are better acquainted with nature than any body, and you well know how difficult it is to follow her in her operations. As to the rest, I shall think myself well rewarded for my trouble, if she more frequently gives me the pleasure of amusing you.

I have the honour to be, with a sincere and respectful attachment,

S I R,

Your most humble and

obedient servant,

W. Mazeas,

*END of PART L*

